

**Disciplinary Rhetorics and Fractal Orderings:
A Study of Sociologies of Knowledge and of Presidential Addresses to
the American Sociological Society/Association**

Robert White B.Sc. B.A. (Hons)

Submitted in fulfilment of the requirements

for the degree of

Doctor of Philosophy

University of Tasmania

June 1998

School of Sociology & Social Work

Cont
Thesis
WHITE
Ph.D
1998



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Declaration

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ABSTRACT

This thesis is a demonstration of the practical achievement of sociological knowledge in the absence of epistemological foundations. Its argument is derived from, and applied to, the disciplinary rhetorics in sociologies of knowledge and presidential addresses to the American Sociological Society/Association. The formal sociologies of knowledge are shown to be representations of discipline, and the addresses, delivered by formal representatives of the discipline, to be 'folk' sociologies of knowledge.

'Discipline' epitomises the location of sociology in the processes studied within it, and of the sociology of knowledge within sociology at large. As subsumed in 'Mannheim's paradox', the sub-/metadiscipline has always been characterised by such epistemological difficulties as circularity, infinite regress, or the apparent self-negation of relativised knowledge. These difficulties, which are also characteristic of the discipline at large, are clarified in Latour's account of the modern as a separation of epistemological from political senses of representation. Sociologists of knowledge have resolved 'Mannheim's paradox' by two recurrent forms of representation, or modes of ordering. Either they relate knowledge to its context in binary terms (which then either axiomatically or tacitly favours the epistemological over the political), or they avoid this preemption through trinary orderings. The deferral of analytical closure in trinitarianism is used in a description of disciplinary knowledge as a 'fractal ordering' of 'disciplinarity', 'disciplining' and 'discipline', where 'fractal' denotes a jagged self-similarity across different scales of study.

In their little-studied addresses to the ASS/A, the presidents are rhetorically required to discuss disciplinary knowledge but are rhetorically constrained from doing so with formal rigour. The addresses are then a practical enactment of 'Mannheim's paradox', and the presidents are folk sociologists of knowledge. Folk and formal disciplinary rhetorics are linked through equation of the 'discipline' from the sociologies of knowledge with 'the discipline' of American sociology, through quantitative matching of 'real factors' from the presidents' organisational characteristics with 'ideal factors' from their addresses, and through use in the addresses of 'disciplinarity', 'disciplining' and 'discipline'.

In demonstrating that isomorphism between sociologies of knowledge and the presidents' occasioned response to occasioned constraints, the thesis amounts to a practical resolution of Mannheim's paradox. The trans-scalar interaction of fractal ordering shows that sociological knowledge is achieved pragmatically in a space defined by its epistemological difficulties.

ACKNOWLEDGEMENTS

To work in the sociology of disciplinary knowledge is to live the collectivity which is at issue. It is a pleasure to acknowledge the generosity of many people who have made this thesis possible.

To Bill Tyler, thanks for getting me started in sociology. To Sandi Hudspith and Warren Bradley, thanks for long-distance cheer and for R & R in Darwin. To the staff of the Morris Miller Library, thanks for everything. To Michelle Gabriel, *Ecco*.

Members of the Department of Sociology and Social Work at the University of Tasmania from 1994 to 1997 have helped in more ways than I can tell. Students in the introductory course, having their first taste of sociological disciplining, have taught me more than I can claim to have done for them. My fellow postgraduates, following their own obsessions, have shown me limits to mine. The staff (and now my colleagues) have been exemplars in ways they might not recognise. My friends in lunch and reading groups have shared the interaction of formal and informal.

Rowena Stewart and Phil Patman have always been unstinting with practical and technical assistance. Smoking may be a health hazard, but the thesis owes more than I know to daily conversations with my fellow nicotine-refugee, Warren Sproule. Rod Crook has been generous with his memories of American sociology. Malcolm Waters encouraged me to come to Hobart, Jan Pakulski suggested the topic, and both have been unfailingly supportive. I could not have wished for a better supervisor than Stephen Crook, whose acute readings and sceptical silences were matched only by his kindness and patience.

The thesis is dedicated to the memory of a friend who would have enjoyed disagreeing with much of it: David Wai Lun Yick, 1955-1996.

CONTENTS

INTRODUCTION	1
Chapter 1: Sociology and Discipline	2
Case 1: Goffman's <i>Dramaturgy</i>	11
PART I: SOCIOLOGIES OF KNOWLEDGE AS REPRESENTATIONS OF DISCIPLINE	17
Chapter 2: Towards a Sociology of Disciplinary Knowledge	18
Actor-Network Theory and Representation	19
An Imagined Tradition - Rhetoric and the Sociology of Knowledge	21
Sociology of Knowledge	26
Scheler	
Mannheim	
Sociology of knowledge: reprise	
Sociology of Science	36
Merton	
Kuhn	
Sociology of science: reprise	
Sociologies of Scientific Knowledge	45
The strong programme	
Discourse analysis	
Sociologies of scientific knowledge: reprise	
Return of Rhetoric	53
Towards a Sociology of Disciplinary Knowledge	57
Actor-network theory and the metadisciplinary	
Subdisciplinary enrolments	
Towards a sociology of disciplinary knowledge: reprise	
Case 2: Wirth's and Gamson's 'We'	64
Chapter 3: Modelling the Disciplinary	71
Disciplinary Science	72
Merton on discipline	
Mertonian common sense, within sociology	
Mertonian common sense, beyond sociology	
Disciplinary science: reprise	
Disciplinary Discourse	78
Foucault on discipline	
Discipline and disciplinarity	
Discipline and governmentality	
Disciplinary discourse: reprise	
Disciplinary Culture	84
Geertz on discipline	
An enrolment of Geertz	
Disciplinary culture: reprise	
Modelling the Disciplinary	89
Disciplinarity	

Disciplining
 Discipline
 Modelling the disciplinary: reprise

Case 3: Gans' Discipline 96

Chapter 4: Disciplinary Methods - Fractal Ordering 101

Real Like Nature - Quantitative Analysis 102

Real factors - Age, period and cohort effects

Real factors - Gender

Real factors - Organisational embeddedness

Ideal factors - Content-analysis

Ideal factors - Citation-analysis

Real like nature - Quantitative analysis: reprise

Narrated Like Discourse - Rhetorical Analysis 114

Genre

The topics

Metaphor

Intertextuality

Narrated like discourse - Rhetorical analysis: reprise

Collective Like Society - Textual Ethnography 123

Ethnography and sociologies of knowledge

Ethnographic reading

Ethnographic writing

Collective like society - Textual ethnography: reprise

Fractal Ordering 128

Fractals and strange attractors

Fractal ordering

Sociologies of Knowledge as Representations of Discipline:

Reprise 132

Formal sociologies of knowledge

Towards a folk sociology of disciplinary knowledge

Case 4: Lazarsfeld's Methodology 136

PART II: 'THE DISCIPLINE' 143

Chapter 5: American Sociology and Presidential Addresses 145

Formalisation of 'The Discipline' 146

'The Discipline' in Crisis ? 148

Sociologies of sociology

Continuing crisis

Permanent crisis

'The discipline' in crisis ? Reprise

'The Discipline' and the Presidency of the ASS/A 155

The presidency and formal organisation

The presidency as documentary of 'the discipline'

'The discipline' and the presidency of the ASS/A: reprise

'The Discipline' and Presidential Addresses 160

Presidential addresses as documentary of 'the discipline'

Presidential addresses as folk sociology of knowledge

Presidential addresses and formal sociologies of knowledge

'The discipline' and presidential addresses: reprise

American Sociology and Presidential Addresses: Reprise 168

<i>Case 5: Coser's, Blau's and Parsons' Genres</i>	170
Chapter 6: The Presidents and Presidential Addresses -	
Real and Ideal Factors	178
The Presidents - Real Factors	180
Age and cohort	
School	
Organisational embeddedness	
Vice-Presidency	
Gender	
The presidents - Real factors: reprise	
The Presidential Addresses - Ideal Factors	190
Content-analysis	
Citation-analysis	
Presidential addresses - Ideal factors: reprise	
Real and Ideal Factors - Correspondence-Analysis	205
Dimension 1 - Discipline and collectivity	
Dimension 2 - Science and knowledge	
Dimension 3 - Discipline and the golden age	
Real and ideal factors - Correspondence-analysis: reprise	
The Presidents and Presidential Addresses - Real and Ideal	
Factors: Reprise	211
'The Discipline': Reprise	212
 <i>Case 6: Merton's Antimetabole</i>	 214

PART III: REPRESENTATIVES OF 'THE DISCIPLINE' AS SOCIOLOGISTS OF KNOWLEDGE

Chapter 7: Folk Disciplinarity	223
Topics and The Topics	224
Folk Collectivity - I	225
Folk Science	227
Imaginaries of science	
Methods of science	
Lawful science	
Contested science	
Folk science: reprise	
Folk Usefulness	234
Imaginaries of usefulness	
Lawful usefulness	
Methods of usefulness	
Contested usefulness	
Folk usefulness: reprise	
Folk Collectivity - II	241
Tradition	
Purity and danger	
Opportunity	
Folk collectivity: reprise	
Folk Disciplinarity: Reprise	245
 <i>Case 7: Taylor's Disciplinarity</i>	 248

Chapter 8: Folk Disciplining	253
Generic Disciplining	254
Topical Disciplining	255
Disciplinary disciplining	
Disciplinary morality	
Topical disciplining: reprise	
Tropical Disciplining - Task	260
Task	
'We' - boundedness	
Sustained devotion - religion	
Intellectual sweat - work	
Tropical disciplining - task: reprise	
Fractal Disciplining - Task	271
Folk Disciplining: Reprise	274
 <i>Case 8: MacIver's Task</i>	 277
 Chapter 9: Folk Discipline	 283
Discipline, Modern, Knowledge	284
Discipline	
Modern	
Knowledge	
Discipline, modern, knowledge: reprise	
Disciplined Knowledge	292
Documents - Rhetoric, language, writing	
Devices - Organisation	
Drilled bodies - Community	
Disciplined knowledge: reprise	
Modern Knowledge	300
Young science	
Paradox	
Represent	
Modern knowledge: reprise	
Folk Discipline: Reprise	308
Representatives of 'The Discipline' as Sociologists of Knowledge: Reprise	310
 CONCLUSION	 313
 <i>Case 9: Coleman's Modernity</i>	 314
 Chapter 10: Disciplinary Rhetorics and Fractal Orderings	 325
 References	 331
 Appendix A - Presidential Addresses	 375
 Appendix B - Real and Ideal Factors	 380

INTRODUCTION

CHAPTER 1: SOCIOLOGY AND DISCIPLINE

The discipline of sociology is an abiding puzzle. Since sociological knowledge of the processes of modernity was from the start an effect of sociology's own emergence from those processes it has always borne a requirement for self-exemplification. As seen particularly in the sociologies of knowledge and in disciplined accounts of the discipline, that disciplinary precondition leads to peculiar effects. Accounts of sociological knowledge are rife with logical monsters. Directed to the problem of order, sociological knowledge has always been an ordering problem in itself. Attempts to establish firm foundations for the discipline have repeatedly been contested, to the extent that the very possibility of sociology appears uncertain. It is then puzzling that sociology continues to exist, and even to flourish. On a first assumption, that 'discipline' epitomises sociology's location in modernity, a practical resolution of that puzzle is developed in this thesis. It must be stressed from the outset that 'resolution' does not imply an end to analytical difficulties; to claim that would be to repeat the problem. Rather, the thesis comprises a pragmatic approach to the permanent and preconditional puzzles of sociological knowledge. Through disciplinary rhetorics in the sociologies of knowledge and in presidential addresses to the American Sociological Society/ Association, disciplinary knowledge will be represented as a 'fractal ordering', where this denotes a jagged self-similarity of patterning across disciplinary accounts of modernity, formal subdisciplinary study of sociology, and disciplinary folk practices. On that trans-scalar and trans-historical continuity, the possibility of sociological knowledge will be seen to derive not from firm foundations but from their absence. Such certainty as disciplinary knowledge commands will be shown to be a pragmatic and provisional effect of uncertainty.

The relation between the sociologies of knowledge and presidential addresses is not self-evident. It will be developed in the thesis from the isomorphic way that each genre is at once sub- and metadisciplinary, and from the difficulties which follow from that dual effectivity. The sociology of knowledge is defined by attention to what Mannheim called the 'existential determination' of knowledge, and ever since he and Scheler formalised it, the field has been both a subdiscipline and a putatively legislative metadiscipline. The sociology of knowledge *is* sociology and it is *about*

sociology, so that the requirement in it for self-exemplification is then obvious. However, the limits deriving from that tension only repeat what is found in sociology more generally. Such subdisciplinary themes as the arational moment in collective life, the interpenetration of formal and informal, or the linking of social order to authoritative knowledge, are also classical problematics in sociology. Despite this, the subdiscipline has at best been intermittently accepted in the discipline at large, while its metadisciplinary claims have been even less attended. This too is more generally the case; while directive potential has been claimed for sociology ever since Comte coined the word, results hardly match ambitions. Since the sociology of knowledge is then to sociology as sociology is to the world at large, it allows a conveniently small-scale access to the tensions of the discipline. Those in the sociologies of knowledge are subsumed in 'Mannheim's paradox'.

To treat knowledge as existentially determined is to relativise it, and, in a standard critique, that is held to be self-contradictory: since the claim itself is absolute, a contention that knowledge is relative is self-defeating. That one breach of logic is accompanied by others: since existential determination is an effect of knowledge while knowledge is an effect of existential determination, arguments within the sociology of knowledge are circular; or since any attempt to find a stable point for analysis can always be questioned, the field leads to an infinite regress. On that epistemological critique, such logical monstrosities are pathological, amounting to a denial of the possibility of disciplined knowledge.

But this paradox is pathological only on the assumptions of epistemology, and, since Mannheim (1922) began his work with a critique of epistemology, to argue epistemologically against the sociology of knowledge is to beg the question. In a standard riposte from within the subdiscipline, that breach of logic is subject to the *tu quoque* argument (cf. Ashmore 1989).¹ Since charge and counter-charge are equally circular, a permanent stand-off ensues. If the sociologies of knowledge are to be self-exemplifying, if they are to be justified sociologically rather than dismissed on epistemological grounds,² if, that is, they are a matter of 'action' rather than of 'ideas', then relativism, infinite regress, or circularity as such cannot be taken as

¹ Ashmore's self-exemplification of the entanglements of reflexivity in the sociologies of knowledge is a comprehensive survey of the recurrence of difficulties in the field.

² The arguments in Popper's (1945) widely-quoted dismissal of the sociology of knowledge are standard (cf. Williams (1989) on Popper's misreading of Mannheim).

definitively pathological. The “idea of self-contradiction”, for example, is “a belated end-product of the practice of science” (Latour 1988a: 157) rather than a given which can be unproblematically invoked. A sociology of knowledge on that account is necessarily a teratology, with the point of interest being how rationality is achieved, or how the inevitability of logical monstrosity is accommodated. This is a practical question,³ and as Mannheim (1925: 62n) said, it entails an insistence that problems be neither denied nor preempted:

we definitely prefer a ‘relativism’ which accentuates the difficulties of its task by calling attention to all those moments which tend to make the proposition actually discoverable at any given time, partial and situationally conditioned - we prefer such a ‘relativism’ to an ‘absolutism’ which loudly proclaims, as a matter of principle, the absoluteness of its own position, or of ‘truth in itself’ but is itself no less partial than any of its adversaries.

There is then no ‘problem of relativism’ in the epistemological sense, but rather a requirement for study of the ways by which relativism is constructed as a problem.

In one form or another, Mannheim’s own approach to the difficulties - treatment of the epistemological subject as always already intersubjective - is characteristic of the sociologies of knowledge. Strict logic is seen within them as a contingent and collective achievement on the part of knowledge-producers. While it may then be a necessary component of the study of knowledge it cannot be a sufficient grounding for it. When both knowledge and its study are seen as emergent rather than as amenable to *a priori* specification, the sociology of knowledge can be grounded only in a pragmatic attention to the pragmatic details of the achievement of rationality.

The approach to intersubjectivity to be used here is adapted from actor-network theory, and in particular from Latour’s (1991) reading of the modern as an effect of the dualisms epitomised in the distinction of epistemological from political senses of representation, and of representation as always rhetorically inflected. These related claims form the second and third major assumptions of the thesis. Just as ‘discipline’ was assumed to be a paradigmatic term for sociology’s location in what is studied within it, so the preemption of difficulties in one dualistic practice is taken to typify the effect of dualism as a form, and thus to be an instance of a characteristic problem within the sociologies of knowledge.

Within the post-structuralism which has been adapted, ‘translated’, in actor-network theory, the foreclosures of binary classification are a central problematic.⁴ It

³ Philosophically, this comes closest to pragmatism (e.g. Rorty 1987).

⁴ Derrida’s (e.g. 1967) is a widely adopted account of it.

is assumed on that basis that to order by twos is to repeat the preemptions of ordering by strict logic. In the third assumption, the stress on practical achievement in classical understandings of rhetoric as a counterpart of logic is taken as epitomising a solution to that problem. When the three assumptions are combined, the practical resolution of Mannheim's paradox derived from them entails attention to dualistic forms of disciplinary rhetorics of representation and the development and use of an alternative. Methodologically, this involves following the actors - sociologists of knowledge - through their textual practices. Since forms of representation are to be examined in them, the sociologies of knowledge are then both topic and resource for the thesis.

As resource, attention to rhetoric in the sociologies of knowledge has ranged from the expressly denied through the tacitly deployed to the openly adopted. Presidential addresses, however, are, by definition, explicitly rhetorical; they are the published versions of ceremonially occasioned speeches. They are linked to the sociologies of knowledge, too, through the disciplinary reflexivity required by that occasioning and through an isomorphism in its realisation, for like the sociologies of knowledge, the addresses are both sub- and metadisciplinary.

On the one hand, the presidency of the ASS/A, the highest formal honour in American sociology, is conferred on those whose work the most established sections of the membership have judged the most eminent, and the address allows each president to elaborate on the methods in, and broader relevance of, the substantive area thus sanctioned as central to the discipline. This is the presidential address *as* exemplary sociology, formally recognised. On the other hand, delivered as a feature of a ceremonial component of the annual meeting, the organisation and the setting of the theme of which is one of the principal presidential duties, to an audience of sociologists and guests from government, business and other disciplines, the address is a ritual state-of-the-union speech, hortatory, celebratory or admonitory in tone, directing attention to failures and achievements in the discipline at large. This is the presidential address *about* exemplary sociology, formally justified. But a familiar tension recurs between those sub- and metadisciplinary moments. In a process which makes their addresses a lived version of 'Mannheim's paradox', the presidents are rhetorically debarred - through the necessity of appealing to a discipline-wide audience - from fulfilling what is rhetorically required of them: that is, the policing of a particular version of the

discipline.

Like the sociologies of knowledge, too, the address are both topic and resource for the thesis. As topic, those to be studied are the sixty published from 1936 to 1995. Although sociology had been taught for some thirty years before formation of the ASS in 1905, the founding of the *American Sociological Review* in 1936 is a convenient starting-point, for ever since Chapin's (1935) address was the lead article in Number 1 of Volume 1,⁵ each has appeared in the *Review*. As resource, the addresses were delivered by those who, by definition, were among the most eminent American sociologists, and whose work has been variously absorbed in the common sense of the discipline; to some extent, it is impossible to do sociology at all without doing the sociology represented in the addresses.

The location of presidential addresses within the ritual of the discipline, and the isomorphism between them and the sociologies of knowledge, might be taken to warrant a contrast between formal and folk disciplinary rhetorics. But to couch the problem in terms of the standard sociological dichotomy between 'formal' and 'informal' would be to repeat the dualisms leading to 'Mannheim's paradox'. A less preemptive ordering principle is required. The mode adopted in the thesis, ordering by threes, was in part located inductively through observation of its recurrence in the sociologies of knowledge and in part derived from such traditional divisions of rhetoric as those between logical, ethical and emotional argumentation or epideictic, forensic and deliberative genres. By their form alone, threefold or 'trinary' classifications are qualitatively different from the binary, with categories within them having an interactive instability. Such categories are well-suited to the pragmatic ordering required once logic is problematised. Their use does not block the effects of relativism, infinite regress or circularity, but rather allows them to be accommodated. It allows, that is, for Mannheim's insistence on the permanence of difficulties.

On that basis, the fixity of the distinctions between the sub- and the metadisciplinary and between topic and resource will be replaced with a modelling of the disciplinary into moments of 'disciplinarity', 'disciplining' and 'discipline'. That modelling is substantively and methodologically applicable to formal studies of disciplined knowledge, and to the folk version of them in the addresses as well. To use

⁵ Curly brackets are used throughout the thesis to indicate the year in which each president held office.

a scheme from classical rhetoric as the central conceit of the thesis, the isomorphism between the two genres is then antimetabolic rather than antithetic: as in the injunction 'eat to live, don't live to eat', it implies an interaction between its poles rather than an opposition. In those terms, formal sociologies of knowledge are representations of discipline, and, delivered by formal representatives of the discipline, the addresses are tacit sociologies of knowledge. Formal and folk are then distinguished on the basis of their occasioning.

That general applicability of disciplinary rhetorics is the processual state described as 'fractal ordering'. Derived from accounts of 'chaos' in the physical and biological sciences, following the same accommodation of logical monstrosities as those in mathematical rationality, and denoting a jagged self-similarity over scale, it is a trope for the achievement of order amidst the perpetual deferral of closure given in the shift from the either-or of antithesis to the interactive both-and of antimetabole. Since it includes synchronic regularities across substantive, methodological and occasioned forms of disciplinary writing and diachronic continuities between sociology and the rhetorical tradition it implies a reconciliation between the provisional stability of sociological knowledge and the instability of its production. It shows, that is, the way that the foundations of sociology are achieved pragmatically from the lack of foundations; sociological knowledge is possible because of rather than despite its epistemological impossibility.⁶

It should be stressed that this is not a critique of sociological knowledge, in the conventional sense of an unmasking. Once study is restricted to following the actors, once such logical pathologies as self-contradiction are treated as effects, and once the requirement for self-exemplification is accepted, any accusatory use of critique is precluded. A sociology of disciplinary knowledge on that basis can be no more than a description of the practices required in a disciplinary with-world, to use one of Scheler's terms of intersubjectivity. It is one ordering among other possible orderings.

Now the precondition of self-exemplification requires that the thesis must itself be an antimetabolic fractal ordering, and that dictates an emergent presentation of the argument. Between this introduction and a concluding section the thesis will be

⁶ Turner and Turner's (1990) reach a similar conclusion in their account of American sociology as 'the impossible science'.

developed in three parts, focused respectively on sociologies of knowledge, on 'the discipline' of American sociology, and on presidential addresses. The three chapters of Part I are necessarily both meta- and subdisciplinary. In the first sense, sociologies of knowledge are shown to be performative representations of 'discipline', and in the latter a set of criteria, models and methods is derived from them for the study of other forms of sociological apologetics. In Chapter 2, responses to 'Mannheim's paradox' will be taken to epitomise the implications of sociology's self-location. That circularity will be broken provisionally through an actor-network theoretical/rhetorical reading of Scheler's and Mannheim's sociologies of knowledge, of Merton's and Kuhn's sociologies of science, and of two sociologies of scientific knowledge, Bloor's and Barnes' 'strong programme' and Mulkey's 'discourse analysis'. Two characteristic rhetorical moves recur in the linking of knowledge with social order throughout these three phases in subdisciplinary history. Such dyadic orderings as base/superstructure, culture/civilisation, or rational/irrational imply foundational claims for sociological knowledge, but they have recurrently been countered with less preemptive triadic classifications. The interaction within these triads allows an epistemically flat sociology to emerge. One triadic homology between Mannheim's, Kuhn's, and Latour's work will be used in Chapter 3 to generate an account of studies of 'discipline', and to derive from them a model of the disciplinary. The disciplinary will be treated as an emergent effect of 'disciplinarity', 'disciplining' and 'discipline', where disciplinarity is an optimistic imaginary of collectivity, science and usefulness, discipline itself is a 'congenitally failing' set of lived practices, and disciplining is the normative linking of the two. That homology will be used again in Chapter 4, to survey 'scientific', 'discursive' and 'textually ethnographic' methods in studies of disciplinary knowledge. The figure of a 'fractal ordering' will be introduced to accommodate a jagged regularity within this chapter, and between it and the previous two. It suggests the trans-scalar similarity of theory, model and method, the self-exemplification needed in a sociology of knowledge, and, again at any scale, its implication in more general orderings. It epitomises the argument that sociologies of knowledge are representations of discipline.

That is one pole of the antimetabole of the thesis. The other is that representatives of the discipline act as sociologists of knowledge. To establish that second pole, the context of American sociology - 'the discipline', as it is commonly

called - must be set, and the place in it of presidential addresses suggested. That is the burden of the two chapters of Part II. In Chapter 5, 'the discipline' will be matched with the 'discipline' from Part I through the recurrence in the specifically American sociologies of sociology of the problematics and problems of the sociologies of knowledge. As documentary of them, the formally representative presidency of the ASS/A will be shown to entail an uncertain representation, and presidential addresses to be an uncertain effect of that. More specifically, it will be seen that while the presidents are situationally required to treat the same issues as do sociologists of knowledge they make scant reference to the formal sub-/metadisciplines, and that their addresses are then, collectively, a folk sociology of knowledge. That claim is confirmed quantitatively in Chapter 6, through a linking of the 'real' and 'ideal' factors from the classical sociology of knowledge. 'Real factors' refer to the presidents' organisational characteristics and 'ideal factors' to features of their addresses, and since their matching is raggedly consistent with the sociologies of knowledge and with accounts of American sociology, a preliminary case will have been made that the presidents are folk sociologists of knowledge.

That is elaborated in the three chapters of Part III, which consist of rhetorical readings of the addresses. The presidents will be shown in Chapter 7 to use a 'folk disciplinarity'; they invoke the collectivity, science, and usefulness modelled from the sociologies of knowledge, and they do so across the expected range of scales. Chapter 8 is devoted to 'folk disciplining'. Three forms of normativity will be distinguished: generic, topical and tropical. The first refers to the presidents' occasioned requirement to police the discipline; this is limited by occasioned constraints. Explicit invocations of disciplinarity in 'topical disciplining' are just as constrained. More commonly, the presidents use 'tropical disciplining', or a metaphorical and intertextual linking of sociological knowledge to other forms and sources of social order; this effect will be illustrated through one recurrent image, that of 'task'. Finally, the 'folk discipline' depicted by the presidents is shown in Chapter 9 to have the 'congenitally failing' character of the formal model. In their implicit repetitions of failings which they explicitly diagnose, the presidents show that disciplined knowledge, modern knowledge, is failing knowledge.

On that demonstration of folk disciplinarity, disciplining, and discipline, the argument of the thesis is complete. The formal sociologies of knowledge are

representations of discipline, the representatives of the discipline are folk sociologists of knowledge, and formal and folk are mutually fractal. That amounts to a resolution of 'Mannheim's paradox'. Antimetabole replaces antithesis, and 'fractal ordering' is a trinary alternative to characteristically binary closures in disciplinary rhetorics. The thesis reconciles two puzzling genres of disciplinary reflexivity, and, through its bootstrapping derivation, is self-exemplifying of the pragmatic possibility of sociology.

The antimetabolic conceit of the thesis also dictates an interactive presentation of the two genres of disciplinary writing under study. Each of the first nine chapters of the thesis is followed by a brief case-study of an address or addresses. This serves to exemplify the circularity of any study of knowledge, and to work within the emergent and mutually fractal orderings of formal and folk sociologies of knowledge. The case studies serve as both counterpoint to and amplification of the developing argument. They have been selected for their relevance to the themes of the chapters preceding them, through a version of Stark's (1972: vii) 'method of significant detail'.⁷ The generality of the points made in each case-study will become cumulatively evident throughout the thesis, and particularly so in the more systematic quantitative and rhetorical detail of Parts II and III. The first of these case-studies, on Goffman's (1982) 'dramaturgy' of the genre of presidential address, is used to foreshadow the thesis as a whole.

⁷ The method "consists in giving for every proposition that is stated, for every assertion that is made, a number of illustrations which, between them, build up a comprehensive and realistic picture. I remember discussing this procedure once with the late Karl Mannheim; he gave his opinion that it is totally appropriate for studies in the area of cultural sociology".

CASE 1 : GOFFMAN'S DRAMATURGY

'[I]n theory', Goffman (1982) wrote in his own, 'a presidential address, whatever its character, must have some significance for the profession, even if only a sad one', and of all the presidents under study he gave the most extended account of that 'significance'.¹ His characteristically phenomenologico-ethnographic 'dramaturgy' of the 'passion of the podium' of presidential addresses allows an epitome of the thesis. First, it will be used to stress the addresses' generic distinctiveness. Then, his 'interaction order' will be shown to be consistent with the moves in the sociologies of knowledge identified in Part I, his disciplinary standing with the reading of American sociology in Part II, and his rhetoric with the folk disciplinarity, disciplining, and discipline treated in Part III. In conclusion, a seeming contradiction between Goffman's dramaturgy of the genre and the use made of his own address will be taken to indicate of a more general interpenetration of formal and folk sociologies of knowledge.

Each presidential address appears as lead article in the *American Sociological Review*, but while it resembles other papers in format Goffman noted that it 'faces one set of requirements, an article in a scholarly journal quite another'. Delivered before 'the largest audience of colleagues that sociology can provide', and, when published, allowing others 'to participate vicariously in what can be read as the culmination of the meeting they missed', an address is occasioned as a 'ritual reaffirmation' of the discipline. This entailed a certain absurdity:

presidents of learned societies are well enough known about something to be elected because of it. Taking office, they find a podium attached, along with encouragement to demonstrate that they are indeed obsessed by what their election proved they were already known to be obsessed by.

Situationally enjoined to see themselves as 'representative of something', presidents acted as temporary 'guardians of their disciplines'. The results could be ridiculous:

However large or oddly shaped the hall, their self swells out to fill it. Nor do narrow disciplinary concerns set limits. Whatever the public issues of the day, the speaker's discipline is shown to have incisive bearing on them. Moreover, the very occasion seems to

¹ Throughout the thesis, page numbers will not be given for quotations from the addresses. In keeping with the emergent argument that the presidents act as folk sociologists of knowledge, the addresses will be treated more as ethnographic data than as texts in the conventional sense. Further, to include standard citations with the substantively necessary references to the presidents by years of presidency rather than by years of publication of their addresses would produce a distracting clutter.

make presidential speakers dangerously at one with themselves; warmed by the celebration, they give without stint, sidetracking their prepared address with parenthetical admissions, *obiter dicta*, ethical and political asides and other medallions of belief. And ... there occurs that special flagrancy of high office: the indulgence of self-congratulation in public.

Nor should the effects of ritual reaffirmation be overestimated, since it was difficult to demonstrate that anything macroscopically significant resulted:

A one-time intersection of variously impinging interests may be represented, and nothing beyond that. Certainly celebrative occasions such as this presidential address don't necessarily have the effect of recommitting the members of the audience to the discipline and profession under whose name they foregather. Indeed, all one can hope for is that memory of how the hour was passed will fade quickly, allowing everyone to attend again the following year, willing once again to not not come.

Goffman's own address went beyond this occasioned absurdity. Having been ready to highlight the gaps between addresses as spoken and as printed, and having expected 'not to publish this talk but to limit it to the precincts in which it was delivered', he was too ill to preside over the meeting of 1982, and in fact died before the address appeared in its conventional place. In a preamble to it, he had held that he was then offering his readers 'vicarious participation in something that did not itself take place. A podium performance, but only readers in the seats. A dubious offering'.

So in Goffman's dramaturgy a presidential address is an 'embarrassing' and reflexive fusion of exemplary sociology and ritual disciplinary reaffirmation. Those generic features and Goffman's own adaptation to them are consistent with this thesis.

In Part I it will be shown that the sociologies of knowledge are characterised by a tension between binary separations of knowledge from the context of its production and trinary alternatives to them. Now Goffman can be read as a sociologist of knowledge. A more general fit between his 'frame analysis' and the subdiscipline is made explicit in the grounding of his dramaturgy in the claim that 'ours is the discipline, the model of analysis, for which ceremonies are data as well as duty, for which talk provides conduct to observe as well as opinion to consider'. His address also shows the strain towards metadisciplinary foundationalism which will be seen as an effect of binary ordering. In his exemplary moment, he had linked his argument to his:

concern over the years ... to promote acceptance of [the] face-to-face domain as an analytically viable one - a domain which might be titled, for want of any happy name, the interaction order - a domain whose preferred method of study is microanalysis.

Since interaction was predicated on 'shared cognitive presuppositions, if not normative ones, and self-sustained restraints', and since its orderliness was evident in any number

of situations, that method in that domain entailed the opening of such sociological dualisms as those 'between village life and city life, between domestic settings and public ones, between intimate, long-standing relations and fleeting impersonal ones'. That is a typical move in the sociologies of knowledge. But, and in a move which is just as typical, Goffman opened one set of binaries by using another. When he held that interaction 'can be identified narrowly as that which uniquely transpires in social situations, that is, environments in which two or more individuals are physically in one another's response presence', he drew a new epistemological line. To be sure, he claimed that he did not:

subscribe to the notion that face-to-face behavior is any more real, any less of an arbitrary abstraction, than what we think of as the dealings between two corporations, or the distributions of felonies across the weekly cycle and subregions of a New York borough; in all these cases what we get is somebody's crudely edited summaries.

But since he still talked of 'body to body' interaction as 'the primordial real thing', he reasserted the dualistic preemption of the real characteristic of the sociologies of knowledge. That allowed his descriptions of published addresses as a chance for those who missed the actual events 'to participate vicariously' in them and his own as a 'dubious offering' since it was not actually delivered.

The context of presidential addresses in American sociology is set in Part II of the thesis. Goffman's stress on 'real' participation in the ASA's meeting fixes that location. The address is certainly of and about 'the discipline, since apart from "that peculiar mixture of with-it Americanese and ironic distance which gives Goffman's style its distinction" (Jameson 1976: 127), the two substantives in 'the interaction order' denote two abiding concerns within American sociology. Part II is developed through the consonance between accounts of fragmentation in 'the discipline' and the model of 'discipline' as congenitally failing developed in Part I. Goffman's obituarists suggested his enactment of that tension. On the one hand, Daniels (1983: 2) depicted him as:

one of the very few sociologists whose work was known beyond the field - and his influence on the world, as well as within the disciplines of social science, has already been so great that he stands as a giant among the social thinkers of his day.

But her noting too that "his resolute refusal to play the game of social manners often drove others into states of real fury" implied a more ambivalent status within the discipline. Dynes (1983: 2) similarly recalled that on his election many sociologists feared that he would "slight" his presidential responsibilities and that "the ASA

staff was initially intimidated by his reputation". In that sense, Goffman enacted the 'failing' of sociology. It will be shown, finally, that presidential addresses entail disciplinary honour, disciplinary policing and disciplinary engagement, and, if only negatively, that trinitarianism is confirmed in Goffman's dramaturgy. His guying of the presidents' self-indulgence, of the ritual of disciplinary representation, and of the claims of relevance to events of the day match those three moments, as does his claim that his own would differ from other addresses 'by virtue of not being particularly autobiographical in character, deeply critical of established methods, or informed by a concern over the plight of disadvantaged groups'.

But Goffman also enacted what he had denied. His address might not have been autobiographical, for example, but it was apologetic. In justifying his attention to 'the interaction order' (and so demonstrating that he was indeed obsessed by what his election had proved he was known to be obsessed by), he referred to the arguments for microanalysis in the prefaces to his books; these required re-statement since other sociologists 'have not been overwhelmed by the merits of the case'. Such apparent contradiction leads to the final part of the thesis.

In Part III, the presidents are shown to be folk sociologists of knowledge, through their use of the same repertoires as those developed in the subdiscipline. Specifically, the addresses will be seen as effects of the disciplinarity, disciplining and discipline of sociology. All that is found in Goffman's address. Both the optimistic imagining of 'science' and its failing - elements of disciplinarity and discipline - are evident in his contention that 'all we can do ... is to keep faith with the spirit of natural science, and lurch along, seriously kidding ourselves that our rut has a forward direction'. The 'usefulness' of sociology appears in a concession to disciplinary engagement:

If one must have warrant addressed to social needs, let it be for unsponsored analyses of the social arrangements enjoyed by those with institutional authority... and all the other well-placed persons who are in a position to give official imprint to versions of reality.

The 'collectivity' infusing 'science' and 'usefulness' appears of course in Goffman's stress on the presidential address as ritual reaffirmation. That linking of imaginary and practice is the process of disciplining; it too appears in the address, and beyond its generic implication. While Goffman claimed that he would not be 'deeply critical of established methods', for example, and that he had 'no universal cure for the ills of

sociology', his defence of microanalysis as allowing access to the 'primordial real thing' was still a normative call for disciplinary renewal. The fusion of disciplinarity, disciplining, and discipline is focused in his peroration:

I'm not one to think that so far our claims can be based on magnificent accomplishment. Indeed, I've heard it said that we should be glad to trade what we've so far produced for a few really good conceptual distinctions and a cold beer. But there is nothing in the world we should trade for what we do have: the bent to sustain in regard to all elements of social life a spirit of unfettered, unsponsored inquiry, and the wisdom not to look elsewhere but ourselves and our discipline for this mandate.

In that disciplining affirmation of an optimistic imaginary in the face of failing Goffman exemplified the standing of the presidents, representatives of the discipline, as folk sociologists of knowledge.

This effect is evident in Goffman's enactment of what he had denied. It will be argued that this is not a contradiction but a constitutive effect of sociological studies of sociological knowledge. The case, moreover, is made in quasi-Goffmanian terms, for the fractal ordering developed from the sociologies of knowledge recall Goffman's (1974) 'frames', and the reading of the addresses requires versions of the 'presuppositions... and self-sustained restraints' which he described as undergirding the interaction order.

For, in a final expression of the interpenetration of formal and folk orderings, Goffman's address refutes his assumption/argument that whatever 'significance for the profession' a presidential address might have is a matter of either direct or vicarious participation in the event of its original presentation. If widespread referencing is any guide, 'The Interaction Order' has been 'significant' for the discipline, but, on an impressionistic judgement, it is usually cited without mention of its occasioning. Each reading of it is then a separate event, inflected, perhaps, by perceptions of the occasion, but not limited to them. So a reading of the addresses for disciplinary presuppositions requires that Goffman's opening of dualism be followed rather than his reassertion of it. It requires that reading and writing be taken as 'social situations' no less than those in which 'individuals are physically in one another's response presence', as no less 'primordially real', as no less open to phenomenologico-ethnographic study than is body-to-body interaction.

That is the inclusiveness elaborated in Part I.

PART I

SOCIOLOGIES OF KNOWLEDGE AS REPRESENTATIONS OF DISCIPLINE

The three chapters of Part I are used to argue that sociologies of knowledge are representations of 'discipline'. A genealogy will be presented in Chapter 2, a model of the disciplinary in Chapter 3, and an ordering of methods for the study of disciplinary knowledge in Chapter 4. Those readings are both meta- and subdisciplinary: a structure of the field as a whole will be drawn from them; and terms will be set for comparison of formal and folk representations of disciplinary knowledge. To conclude that first pole in the antimetabole of the thesis, the distinction between meta- and subdisciplinary will be collapsed, and representation of knowledge depicted as 'fractal ordering'.

CHAPTER 2 : TOWARDS A SOCIOLOGY OF DISCIPLINARY KNOWLEDGE

Sociologies of knowledge are necessary as resources in the study of disciplinary knowledge and as topics in their own right. Attempts to link the known to the contexts of knowing and the conditions of knowers entail processual effects which are characteristic of the discipline writ large, and if the sociology of knowledge is to sociology as sociology is to the world at large, the sub-/metadiscipline allows both the means for study of disciplinary knowledge and a typical case of such knowledge.

Since the circularity between resource and topic entails a form of 'Mannheim's paradox', a reading of the field requires a sociology in which 'paradox' is not barred. Actor-network theory meets that need, and this chapter is structured around it. It will be introduced first, with reference to Latour's (1991) depiction of the modern as the distinction between political and epistemological 'representations', and to the 'orderings' by which Law (1994) resolved the resultant 'strain towards dualism'. The consonance of their stress on textual constructions of knowledge with the rhetorical tradition, in which the epistemic was always already political, will then lead to a rhetorical genealogy of the sociology of knowledge, and that in turn to a reading of three phases in the sub-/metadiscipline. A characteristic set of antitheses recurs in Scheler's and Mannheim's sociologies of knowledge, in Merton's and Kuhn's sociologies of science, and in the strong programme and Mulkay's discourse analysis in the sociology of scientific knowledge. Binaries like rational/ irrational, civilisation/culture, or base/superstructure entail the 'work of purification' which Latour held as marking the modern and which warrant the sub-/metadiscipline being treated as a representation of 'discipline'. But an alternative to them also recurs, trinary ordering. After revival of the rhetorical tradition has been fitted to recent sociologies of knowledge, trinitarian ordering will be presented as an accommodation of 'Mannheim's paradox'. Replacement of the mutually exclusive categories of analysis with interpenetratively emergent orderings allows work within the circularities of relativised knowledge which avoids the preemptions of binaries. In

particular, an homology between Mannheim's triadic sociology of knowledge, Kuhn's trinary 'disciplinary matrix', and Latour's (1991: 6) description of networks as "simultaneously real like nature, narrated like discourse and collective like society" will be taken as the non-foundational foundation for the sociology of disciplinary knowledge developed throughout the thesis.

ACTOR-NETWORK THEORY AND REPRESENTATION

If the sociologies of knowledge are to be treated sociologically rather than philosophically,¹ and if access to them is always textual, then an approach to and within them is needed in which writing is taken as performative. The 'actor-network theory' which Callon, Latour, Law and their colleagues have developed in their accounts of the co-emergence of scientific knowledge, technology, and the social meets that need.²

Drawing on post-Kuhnian sociologies of scientific knowledge and on Serres' philosophy of science, adapting such post-structuralisms as semiotics and Foucauldian discourse analysis, and fusing these through a Machiavellian and pragmatic symbolic interactionism, the actor-network theorists have elaborated a process - 'translation' - "in which putative agents attempt to characterize and pattern the networks of the social: ... in which they attempt to constitute themselves as agents" (Law 1994: 101). Subsuming all the means by which actants both establish themselves as 'obligatory passage points' and 'enrol' allies in the construction and maintenance of politico-scientific networks of association (Callon 1980; 1995), 'translation' is then a mechanism of representation. Claims to authoritative knowledge depend on the extent to which knowledge-producers can sustain their self-appointed status as spokespeople for any number of animate and inanimate constituents. Since these will at the same time be doing their own translating, the process is always as precarious as shown in Callon's (1986) account of the 'betrayal' by the fishermen and scallops of St Brieuc Bay of the scientists acting as their representatives. This exemplifies, too, the performativity of representation. An account

¹ Frisby's (1983/1992) account of Scheler, Lukacs and Mannheim, or Pels' (1996) call for a reconciliation of Mannheimian and Wittgensteinian traditions are recent examples.

² In Lynch and Bogen's (1997) terms, it yields an 'epistemically flat' sociology.

of a translation is not a neutral depiction, but is another politico-epistemic effect; it is itself a translation, in which the writer represents the actants in a particular case, on the one hand, and attempts to enrol the reader on the other. So while this might imply 'methodological horrors',³ that is precisely the circularity required for a sociological reading of sociology.

The networks, or 'translation chains', constructed from representations are heterogeneous, consisting, for example, of 'documents, devices and disciplined bodies' (Law 1986b). The 'relational materialism' which inclusion of these disparate elements requires is a 'modest sociology': of practices rather than 'ideas', of verbs rather than nouns, of ordering rather than order. It is symmetrical (no element of a complex under study is privileged *a priori*), non-reductionist (since attribution of causality to one element breaches symmetry), recursive (both the social and the study of it are treated as emergent), and reflexive (being as applicable to analysts as to those they analyse). Since the last three are corollaries of the first, the principle of symmetry is critical. The open-ended orderings deriving from it are a departure from the dualisms marking modernity, but not in a sense which is itself dualistic:

To turn away from dualism doesn't mean that we should ignore the ordering strains *towards* dualism built into the modern project. Instead, we should seek to treat dualism as a social *project*, a sociological topic, rather than treating it as a resource. Accordingly, the argument is that modernism more or less successfully (though partially and precariously) generates and performs a series of such divisions (Law 1994: 138).

Obviously dualism as such is not definitive of the modern - its roots are far too deep in the western tradition - but the suggestion remains that particular binaries are. That had been the burden of Latour's (1991) argument that 'we have never been modern'.

The very word 'modern' breaches the requirement for analytical symmetry.

When either it or such derivatives as 'modernisation' or 'modernity' are used,

we are defining, by contrast, an archaic and stable past. Furthermore, the word is always being thrown into the middle of a fight, in a quarrel where there are winners and losers, Ancients and Moderns. 'Modern' is thus doubly asymmetrical: it designates a break in the regular passage of time, and it designates a combat in which there are victors and vanquished (Latour 1991: 10).

Latour depicted two sets of practices in this process: in 'work of translation', new types of

³ Woolgar (1988a: 32-33) described the (ethnomethodological) indexicality, inconcludability, and reflexivity of representation in those terms.

beings, hybrids of nature/culture, are created; in 'work of purification', an ontological distinction is drawn between 'human' and 'non-human'. 'Modernity' is enabled by hybrids such as Boyle's air-pump, since the vacuum produced by it in the laboratory "simultaneously permits the definition of the Laws of Nature, the action of God, and the settlement of disputes in England at the time of the Glorious Revolution" (Latour 1991: 42; cf. Shapin and Schaffer 1985). But at the same time modernity entails denial of that hybridisation, through the separation of the epistemological from the political sense of representation, and thus a privileging of both 'nature' and 'culture'.

The processual continuity of the modern with what it supposedly displaced is given in the actor-network theorists' stress on the performativity of written language: 'documents' are a critical element in Law's (1986b) 'heterogeneous engineering'; texts are among the 'immutable mobiles' by which translation at a distance is possible (Latour 1987: 227); and science itself is "a vast enterprise of writing" (Callon 1995: 51). More specifically, both actor-networks and the 'work of purification' to which they offer an alternative are rhetorical achievements, and rhetoric itself, that millennia-long and "fascinating albeit despised discipline" (Latour 1987: 30), was always already both political and epistemic. Rhetoric allows a circular reading of the sociology of knowledge's location in what is studied within it.

AN IMAGINED TRADITION: RHETORIC AND THE SOCIOLOGY OF KNOWLEDGE

An immediate problem arises in the sociology of knowledge: where to start. In a sense, the question is trivial, for the term itself can be dated,⁴ as can the first texts in it. But 'origins' are contestable retroscriptions.⁵ In keeping with Latour's (1991: 76) version of a standard diremption of dualism - "[o]ne is not born traditional; one chooses to become

⁴ It appears to have been first used in English in 1909, when Wilhelm Jerusalem sketched in a letter to William James a reconciliation of Durkheimian sociology and pragmatism (Stikkers 1980: 24). The term *Wissensoziologie* from which it was derived is only a little older.

⁵ Any attempt to fix origins would be to continue that "romance with 'ends' and 'beginnings'" which Crook (1991: 218) found characteristic of modernist foundationalism.

traditional by constant innovation",⁶ a rhetorical genealogy for the sociology of knowledge will be presented here as a basis for a reading of work in it.

When Scheler and Mannheim first formalised it, the sub-/metadiscipline was already all but explicit. Durkheim's 'collective representations', Toennies' distinction of *Gemeinschaft* from *Gesellschaft*, Weber's 'elective affinity', and Simmel's 'stranger' were all versions of it.⁷ Or, it might be taken as co-extensive with sociology itself. Comte (1830-42) assumed the existential determination of knowledge in his law of three stages, as did Marx and Engels (1843: 64) in claiming that "[t]he ideas of the ruling class are in every epoch the ruling ideas". While Marx (1859: 425) was not yet a sociologist (he did not become so until well after his death), let alone a sociologist of knowledge, he set what would become a standard point of departure: "[i]t is not the consciousness of men that determines their existence, but their social existence that determines their consciousness". But of course Comte and Marx had predecessors. When Vico (1744) linked the 'poetic wisdom' of language with institutional change, when Pascal (1659: 46) mocked the claims made for natural law - "It is a funny sort of justice whose limits are marked by a river; true on this side of the Pyrenees, false on the other" - or when Descartes (1637: 39) held that "our beliefs are based much more on custom and example than on any certain knowledge", they too stressed existential determination. Or again, Bacon (1620) has been taken as a precursor, in both his general stance and his specific

⁶ As Latour noted, this is a commonplace. It appears, for example, in Shils' (1981) depiction of traditions as exigent resources, in Hobsbawm's (1983) 'invented traditions', in Lowenthal's (1985) 'past is a foreign country', in Connerton's (1989) account of 'how societies remember', or in Fentress and Wickham's (1992) 'social memory'. Kuhn's (1959) 'essential tension' between tradition and innovation in science is a more proximate referent.

⁷ Durkheim and Mauss (1903: 82) held that "[f]ar from it being the case ... that the social relations of men are based on logical relations between things, in reality it is the former which have provided the prototype for the latter". Durkheim (1912: 435) elaborated the sense in which he meant that 'concepts' were 'collective representations': "If they belong to a whole social group, it is not because they represent the average of the corresponding individual representations; for in that case they would be poorer than the latter in intellectual content, while, as a matter of fact, they contain much that surpasses the knowledge of the average individual. They are not abstractions which have a reality only in particular consciousnesses, but they are as concrete representations as an individual could form of his own personal environment; they correspond to the way in which this very special being, society, considers the things of its own proper experience". As in Loomis' (1955) elaboration, Toennies' (1887/1935) binary can be read as a sociology of knowledge. Weber's 'elective affinity' was "the decisive conception by which [he] relates ideas and interests" (Gerth and Mills 1946: 62). Embodying "nearness and distance at the same time", Simmel's (1908b: 406) 'stranger' enacts as well what is required of a sociologist of knowledge.

theory of the 'idols [which] beset the human mind'; Vico, Comte, Scheler and Mannheim all enrolled him.⁸

The sociology of knowledge is then constitutively rhetorical. Just as Comte took Bacon's work as a 'rallying point', so Bacon, that "peer of scientific propagandists" (Merton 1938a: 43), exemplifies location of the study of existential determination in the much older tradition of sophism and classical rhetoric. That location includes the circularities of the sub-/metadiscipline, for when Bacon opposed induction from the results of observation and experiment to scholastic deduction, he distinguished science from rhetoric with all his rhetorical skill (e.g. Jardine 1974; Bender and Wellerby 1990; Heckel 1991). Now Speier (1937: 164) described the sophists as "the first sociologists of knowledge in Mannheim's sense", while Bacon (1620: 389) held that "the doctrine of idols bears the same relation to the interpretation of nature as that of the confutation of sophisms does to common logic". The difficulty of reconciling a continuity from Bacon's theory of idols to Mannheim's of ideology with those two claims enacts the agonistic co-emergence of what is to count as rational knowledge and how it is to be studied.

That tension appears in Plato's denial of the politico-epistemic doubts assumed by Sophist rhetoricians such as Gorgias, Protagoras or Isocrates. Where they argued from probability, he looked to certainty. Where they claimed that a rhetor should be able to defend either side of any case, he took it as obviously absurd that contraries could be argued. Holding that rhetoric required appeals to prejudice, he saw it as only a panderer's knack when compared with the true philosophy, not worthy of the name of 'art' used by its apologists: "it has no rational account to give of the nature of the various

⁸ "The idols of the tribe are inherent in human nature ... The idols of the den are those of each individual ... The idols of the marketplace are formed by the reciprocal intercourse and society of man with man ... The idols of the theatre have crept into men's minds from the various dogmas of the peculiar systems of philosophy" (Bacon 1620: 390-1). Vico (1744: 67-8) claimed that he was "following the best ascertained method of philosophizing, that of Francis Bacon, Lord Verulam, but carrying it over from the institutions of nature ... to the civil institutions of mankind". Comte (1830-1842 I: 5-6) placed Bacon among the founders of positivism: "It is difficult to assign any precise date to this revolution in science ... But if we must fix upon some marked period, to serve as a rallying point, it must be that - about two centuries ago - when the human mind was astir under the precepts of Bacon, the conceptions of Descartes, and the discoveries of Galileo". Mannheim (1929a: 61) saw Bacon's theory of the 'idols' as "to a certain extent a forerunner of the modern conception of ideology". Scheler made similar use of it, although he did object to the "narrow, all-too-English practical restriction" in Bacon's work (Scheler 1925: 118).

things which it offers. I refuse to give the title of art to anything irrational" (*Gorgias*: 46). Truth lay only in the definitions and divisions of dialectic, and unless rhetoric was subordinated to it, it was at best worthless, and, given the importance of argumentation in civil life, at worst pernicious (cf. *Phaedrus*: 147-8). The entanglement of the rational in the irrational is evident in this, to the extent that Plato's assault on rhetoric has been called a great rhetorical achievement (Vickers 1988: 83-147).⁹ But this pattern was never uncontested, for Aristotle's treatment of rhetoric, a model until at least the Renaissance, is a reconciliation of Sophistic and Platonic positions.

The opening words of his tendentiously titled 'Art' strike a counter-Platonic note: rather than being inferior, "Rhetoric is the counterpart of Dialectic" (*Rhetoric* 1.1.1354a). Rhetorical arguments from examples or enthymemes were correlates of the induction or syllogistic deduction of dialectic, and valid 'proofs' could be derived from ethical or emotional appeals no less than from logic. Aristotle likewise defended the Sophists' contention that a rhetor should be able to prove either side of any case, and endorsed the claim that conviction was achieved through probability rather than certainty. He allowed for Plato's critique by defining the art analytically: "its function is *not* persuasion. It is rather the *detection of the persuasive aspects of each matter...*" (*Rhetoric* 1.1.1355b). He thus treated rhetoric less as a set of techniques - although his *Art* does contain these - than as a meta-theory in which any stark opposition between the rational and the irrational was eschewed. In ranking rhetoric with dialectic, for example, he endorsed what might be called an inverse law of excluded middle, since enthymemic logic depends on omission of the second term of a syllogism. In place of syllogistic compulsion, the enthymeme not only allows but requires an audience to link the first and third terms through its stock of common sense; it is then a binary which is rationally constrained yet open to the arationally lived.¹⁰ Rhetoric was a necessary supplement to the peculiarities - such as the 'Cretan paradox' ¹¹ - of strict logic. Via commentaries and elaborations by Cicero, Quintilian and any number of others, this

⁹ Bacon (1605: 178) had made a similar point: "it was great injustice in Plato, though springing out of a just hatred of the rhetoricians of his time, to esteem of rhetoric but as a voluptuary art".

¹⁰ In an ancient image, logic entails argument with the closed fist, rhetoric with an open hand.

¹¹ In warning against 'idle talkers and deceivers', one of whom, a Cretan, had said 'Cretans are always liars', St Paul (Titus 1: 10, 12) took this as a commonplace.

understanding of the art persisted until the Renaissance. Taught in the emerging universities, it was a standard component of the practice and analysis of knowledge.

But the Baconian and Cartesian rejection of Aristotelianism entailed a revival of Plato's distinction between *doxa* and *episteme*. Rhetoric was disqualified by "the idea of unicity of truth" (Perelman 1979: 12) in the science of the time, whether this was imputed to the inductive treatment of observation or to the deductive certainty of geometry. As exemplified in Hobbes' dispute with Boyle, the epistemological and political senses of 'representation' were separated, and social order was argued through denial that social order was at issue (Shapin and Schaffer 1985; Latour 1991: 27-9). So this genealogy has returned to one point of departure within it, the effects of Bacon's rhetorical anti-rhetoric. It sets a continuity of the modern with the pre-modern; Bender and Wellerby (1990: 8) have compared his stance with Plato's, and Vickers (1988) has said the same of Locke, the writers of the French Enlightenment, and Kant, all of whom opposed to rhetoric their accounts of rationality.¹² Claims for the rational against the irrational all required the arational; all then required 'work of purification'.

Once the sociology of knowledge is placed in that genealogy, with 'Mannheim's paradox' being the 'Cretan paradox' revisited, the same work should be expected in it. Mannheim (1936a: 284, emphasis added) claimed that "[e]very complete and thorough sociological analysis of knowledge delimits, in content *as well as structure*, the view to be analysed", Latour's (1987) prime methodological principle is to 'follow the actors', and Law (1994) took dualism as a sociological topic. These, and the imagined tradition, yield a means of reading work in the sub-/metadiscipline. It is to be expected :

- that accounts of the 'existential determination' of knowledge will be focused on the relation of the rational to the irrational;
- that they will be couched in terms of the scientific versus the non-scientific;
- that representations of knowledge will be political representations;
- that 'work of purification' will be evident in responses to relativism and in rhetorical denials of rhetoric;
- and that that rhetoric will be antithetically structured.

¹² It might be recalled that Kant dedicated his *Critique of Pure Reason* to Bacon.

A reading of those terms is empirical, being directed not to the 'ideas' but to structures of social action in the sociologies of knowledge.¹³ The study of knowledge entails abiding practical difficulties, and there is a transhistorical continuity in solutions to them. Those structures of rhetorical action will be examined in three phases, the first of which is the classical formulation.

SOCIOLOGY OF KNOWLEDGE

In setting the substantive concerns of study of the relation between knowledge and social conditions, Scheler and Mannheim also set what would become recurrent moves in its writing. Kettler and Meja's (1995) account of Mannheim 'rationalizing the irrational' could be applied as well to Scheler. They both worked under the Weimar Republic, when the relation of formal knowledge to social order was acutely at issue, and both wrote in self-consciously political terms; both offered resolutions of relativism; and both their accounts of knowledge are characterised by the expected 'strain towards dualism'. Having accepted circularity, they exemplified the difficulties and possibilities to which it gives rise.

Scheler

The rationalising strain in the sub-/metadiscipline is evident from the start. When Scheler, a philosopher rather than a sociologist, wrote the first prescriptive texts in the field, he assumed that "a pure sociology of knowledge ... is part of the philosophic discipline of epistemology" (Scheler 1921: 163, emphasis removed). The association of purity and rationality was a given. In a phenomenologically Pascalian reading of Toennies, Marx and Comte, Scheler posed three axioms for a sociology of knowledge. First, "[a]ll human knowledge, insofar as man is a 'member' of a society in general, is not empirical but '*a priori*' knowledge" (Scheler 1925: 67). In the 'stable life-community' of Toennies' *Gemeinschaft*, and even in the more utilitarian *Gesellschaft*, the world was always a 'with-world'. The phenomenologically accessible 'relative natural world

¹³ It is empirical, that is, in the sense Parsons (1937: 697-8) claimed for his reading of Marshall, Pareto, Durkheim and Weber.

view' of any individual was then "essentially the intuition of a human 'community'" (Scheler 1913-4: 168).

Secondly, "[e]mpirical participation of a human being in the experience of his fellow human beings realizes itself *in different manners*, depending upon the essential structure of the group" (Scheler 1925: 68). Linking his own ontologies of world and of spirit (e.g. Scheler 1913-14) with Marx's base and superstructure, he held that for empirical access to 'essential structures' "we have to distinguish between a *sociology of culture* and a *sociology of real factors*" (Scheler 1925: 34). Since the 'ideal factors' in cultural institutions bear a contingent relation to such 'real factors' as geographical features, racial inheritance, or economic conditions, this was:

a provisional division to the extent that sociology's ultimate and proper task consists in examining the kinds and the orderly sequence of the *reciprocal effects* of ideal and real factors, as well as of the spiritual and drive factors that determine the contents of human life, which is always socially conditioned by nature (Scheler 1925: 36).

The two sets of factors were then autonomous in their sources but interpenetrative in their effects. Describing the 'real factors' as opening and closing the sluices of the spiritual stream,¹⁴ Scheler (1925: 54) held that they circularly affect which of the ideas or cultural impulses developed in the superstructure would impact on or result in new 'real factors'. A distinction between 'group-mind' and 'group-soul' followed from this. 'Group-soul' denoted the collective subject of such psycho-social activities as were less 'spontaneous' than the automatic or quasi-automatic effect of current 'real factors'; any expression of the phenomenological 'relative natural world view' was an effect of it. But where it was impersonal in origin and anonymous in articulation, the 'group-mind' appeared only in personal terms; it referred to the collective subject which "constitutes itself through conscious *spontaneous* acts of co-experience, intentionally related to their objects" (Scheler 1925: 70). This was the realm of the educated, self-conscious elite, the generators of ideal factors. The group-soul, it might be said, operated from below, the group-mind from above.

Scheler (1925: 70) stressed the regularity of interaction between the two in his

¹⁴ Weber (1915: 280) had proposed a reverse effect: "Not ideas, but material and ideal interests, directly govern men's conduct. Yet very frequently the 'world images' that have been created by 'ideas' have, like switchmen, determined the tracks along which action has been pushed by the dynamic of interest".

third axiom:

a *fixed law* ... orders the origin of our knowledge of reality, i.e. our knowledge of what generally 'brings about effects', and orders the fulfilment of the individual spheres of knowledge, constant in human consciousness, and the correlative *spheres of objects*.

That law was a corrective to the 'gross errors' which Comte had committed, in both order and social location, when he divided history into religious, metaphysical and positive stages. These forms of knowledge - of salvation or redemption, of culture, and of practical matters - had "from *their very origin* differentiated themselves from the rudiments of natural- and historical-mythical thinking and viewing [and] then took a largely distinct line of development" (Scheler 1925: 44). Rather than successive, the three forms of knowledge were permanently coexistent, but with one or another more cogent at any time.¹⁵ Since the 'with-world' of their effects preceded all knowledge of distinctions between them, any classification of what was knowable in a given society must be at least co-conditioned by the divisions in it. As in Levy-Bruhl's Durkheimian theories, there must be a correspondence between social structure and social knowledge.

Knowledge-of-salvation, supplementing ideal-factors/knowledge-of-culture and real-factors/practical-knowledge, allowed a lawful resolution of the relativism which that implied. In the *ordo amoris*, which Scheler (e.g. 1914-16) held as ground and vehicle of all his analyses, he claimed to have established phenomenologically that there is an:

eternal *hierarchy of values*, and its rules of preference are *fully as objective and clearly 'evident'* as mathematical truths. There does exist an *ordre du coeur* and a *logique du coeur* (in Pascal's words) which the moral genius gradually uncovers in history, and it is eternal - only its apprehension and acquisition is 'historical' (Scheler 1915: 72-3).

All cognition was then morally preconditioned. Similar tenets in Troeltsch's or Mannheim's historicist accounts of knowledge led to relativism, as did Durkheimian classifications. By contrast, Scheler (1925: 41-2) held that the absolute morality of his Pascalian *ordo* could prevent analytical chaos, by:

lifting up the absolute *realm of ideas and values* corresponding to the essential idea of man far above *factual* historical value systems; for example, by viewing all orders of goods, ends, and norms of human society in ethics, religion, law, and art as simply relative and as conditioned by a historical and sociological standpoint, preserving nothing but the *idea* of the eternal objective *Logos*.

¹⁵ "Religion, metaphysics, and positive science rest on three different motives, on three entirely different groups of acts of the knowing mind, three different aims, three different personality types, and three different social groups. Also the historical forms of movement of these three mental powers are essentially different" (Scheler 1921: 165).

That objectivity was the ground of lawful purity for the sociology of knowledge.

The tension that recurs throughout this chapter has been set in this sketch. The relativism following from the contingent interaction of ideal and real factors must be rejected, on the standard argument of self-refutation (e.g. Scheler 1925: 51); rationality conditions the sociology of knowledge. But at the same time, the dualistic variations on base and superstructure - world and spirit, real and ideal factors, group soul and group mind, sociologies of real factors and of culture - are disrupted in the triadic interaction between religious, positive, and metaphysical knowledges; the irrational conditions the rational. With a shift of emphasis, that tension recurs in Mannheim's work.

Mannheim

While Mannheim's sociology of knowledge was eclectically sourced within and beyond the emergent sociological tradition, its central features are clear in his objection to two aspects of Scheler's. One was the *ontological* distinction between 'ideal' and 'real' factors; where superstructure and base were treated dialectically in historicism, Mannheim (1925: 83) held that Scheler could link them only through the contradictory positing of a supra-temporal 'essence of man'. In the other, he met Scheler's charge of self-refutation by claiming that analysis of knowledge as perspectivic need not detract from its validity here and now. If the phenomenologically given was seen as dynamic rather than as atemporally static, then there was "an existentially determined truth content in human thought at every stage of its development" (Mannheim 1925: 101). By denying that, Scheler placed himself above history: "according to our view, God's eye is upon the historic process (i.e. it is not meaningless), whereas Scheler must imply that he looks upon the world with God's eyes" (Mannheim 1925: 103). As noted earlier, Mannheim (1924: 130) rejected the "classic anti-relativist argument, repeated *ad infinitum* and *ad nauseam*", and resisted premature closure through his essayistic approach to problems. He complained, for example, that von Schelting (1936), in a review of *Ideologie und Utopie*:

suppresses the fact that the author expressly states that he is on a search and that several systems are at work in the same person, and that this is why - with the new methodology of 'experimental thinking' - he does not touch upon the contradictions that consequently

come into view (from a letter to Louis Wirth, quoted in Kettler and Meja 1995: 212).

But since Mannheim did not elaborate his experimental method, his anti-systematic analysis of the knowledge deriving from the “mutually interdependent parts of a systematic totality” (Mannheim 1925: 68) requires reconstruction.

In the light of his own advice to examine the structure as well as the content of knowledge, his method is read here as an opening of the binaries in Scheler’s work. As he said of the question as to whether he was spiritualist or materialist, “I expressly reject this antinomy, which I find imposed upon me” (Mannheim 1928b: 103). Instead, in his critical, programmatic, and substantive essays in the 1920s, he repeatedly used trinary rather than binary orderings. He set the pattern in his early structural analysis of epistemology. Holding that philosophical accounts of knowledge were inadequate, since the logical, psychological, and ontological precommitments required for them led to a begging of the question, he proposed a typology by which they could be treated as themselves emergent: a triadic relation between the knower (subject), the known, and the to-be-known (object). Mannheim (1922: 58) claimed that “[e]very epistemological systematization is based upon this triad, and every conceivable formulation of the problem of knowledge is given by these three terms in some combination”. Although he distinguished his ‘systematisation’ from Kant’s architectonic ‘fetish’ for trichotomies (Mannheim 1922: 26), and although he did not make it explicit in his later essays, such triadic ordering appears to have been his own architectonic principle. It will be shown here in the genealogical, methodological and political moments of his sociology of knowledge, as set out in Figure 2.1.

Figure 2.1 Trinary structuring of Mannheim’s sociology of knowledge

Genealogical	Methodological	Political
Self-relativisation	Positive science	Liberalism
Ideological unmasking	Historicism	Socialism
Emergence of the social	Phenomenology	Conservatism
Total relativisation	Sociology of knowledge	Dynamic synthesis

Genealogically, Mannheim located the sociology of knowledge in the modern. For all the limits of epistemology, the self-relativisation in it was crucial, being part of that individualising trend in Western philosophy which ran from Descartes' *cogito* to Kant's account of the role of the subject in all thought and which was then partially constitutive of any knowledge under study. It had been followed, interactively, by Marxist ideological analysis and by the emergence of 'the social' as an "ontological 'terminus' of the motion transcending theoretical immanence" (Mannheim 1925: 68) deriving from generalisations of Marx's work.¹⁶ (Mannheim's (1927b) own treatment of 'generations' as 'social locations' exemplified the way that 'class' could be extended). Those three genealogical moments were fused in the 'total relativisation' of the sociology of knowledge.

Mannheim (1921-2; 1924; 1925) likewise depicted it as comprising three sets of methods. It was scientific; in both the "bourgeois nuance" of Durkheimian work and the "proletarian nuance" of materialism, positive sociology had remained at "a relatively primitive level" (Mannheim 1925: 76), but was valuable, ironically, for its metaphysics of 'essential contact with reality'. The study of knowledge also required an 'historical cross-section' through the *Weltanschauung*, or unified complex of meanings prevailing at a given time (phenomenologically accessible through the smallest details of cultural products), and 'historical vertical analysis' (entailing the same circling between parts and whole). Here Mannheim made his replacement of binaries most explicit. Adopting Alfred Weber's distinction between 'culture' and 'civilisation', he held that the former may be treated as a *Gestalt*, open to hermeneutic understanding, the latter as a focus of causal analysis, and so suited to the linearly progressive terms of the Enlightenment. But between culture and civilisation lay a third domain, where 'progress' was not linear but dialectical. Since this differed from the immanent logic in technology or science, historicism entailed analysis of three types of 'developmental sequence':

the *psychic development* which can be represented only as a concrete *Gestalt*; the *dialectical*, in which certain rationalized fields organize themselves anew around new systematizing centres; and *progressive evolution*, in which it is one and the same system which is gradually being built up (Mannheim 1924: 116).

In that triadic schema, then, the irrational (as lived) was held in tension with the

¹⁶ Mannheim relied far more on Lukacs (1923) for this reading than he did on Marx himself.

rational (as studied).

Finally, and on the assumption that nothing becomes an intellectual problem unless it is first a problem of practical life, Mannheim based his sociology of knowledge in political movements. As in the interdependent emergence of liberalism, socialism and conservatism (Mannheim 1927a), and in the conflicting accounts within them of the relation of theory and practice (Mannheim 1928a: 248-50; 1929b: 117-46), knowledge and its study were given as triadically interactive and politically consequential. The sociology of knowledge was a 'dynamic synthesis' of the three movements.

On this reading, Mannheim's 'experimental thinking' had an architectonic unity transcending its essayistic form, being systematic in the "continually receding" avoidance of foreclosure which Mannheim (1928a: 256) achieved through the trinary orderings of his epistemological, genealogical, methodological and political moments. His work was then not so much a legislative prescription as the delineation of a 'range of sensitivity' or 'schema of orientation', in which the rational and the irrational were intermerged at every point. But he did not sustain this tension, and especially so after his move to England (cf. Kettler *et al.* 1984). Despite his embrace of the difficulties of relativism, his account of it had always been uncertain. To purify what he came to accept as the pathological 'total relativisation' of his genealogy, he revived a privilege he had previously accorded to science: total relativisation did not include science, and scientific knowledge should then be politically authoritative. From trialism he returned to the rejected dualism.

While Mannheim (1924: 130-1 n1) had granted that his exemption of scientific knowledge from existential determination may have been existentially determined itself, he nevertheless retained it in his methodological and political moments. Phenomenologically and historically, the objects of inquiry in the natural and cultural sciences were said to differ. The "empirical object given in the concrete fullness of actual sensual experience" (Mannheim 1921-2: 10) presented no problem to a physicist, but objects of social inquiry could be identified only through a self-conscious elaboration of the analyst's own prior concepts; the natural world yielded 'objective' knowledge while the

cultural was accessible only through 'documentary meaning'. Findings in the sciences could then be treated "as direct progress toward ultimately 'correct' knowledge which can be formulated only in one fashion" (Mannheim 1925: 95). Since historical analysis was "not timeless like mathematical or scientific knowledge" (Mannheim 1921-2: 36-7), the latter must be more authoritative. Finally, social and natural scientific knowledges served different political ends. While the former was normative, it was self-evident that "science (in so far as it is free from evaluation) is not a propagandistic device and does not exist for the purpose of communicating evaluations, but rather for the determination of facts" (Mannheim 1931: 296). To be sure, Mannheim (1925: 73-4) granted that scientific knowledge might be only relatively free from evaluations, and scientific progress only relatively immanent, but the control deriving from the quarantining of positive knowledge was still a model for cultural study. Approximating his account of the development of scientific facts, Mannheim (1924: 130) held that in any complex of meanings "only one perspectivic conclusion can be correct".

One group was uniquely qualified to reach that 'total perspective': in Alfred Weber's phrase, the 'relatively unattached intellectuals'. Mannheim stressed that these were neither outside the political arena nor exempt from locational effects. The intellectuals may - indeed, to fulfil their mission, they must - come from a range of backgrounds. But without altogether shedding their origins, they were united by an education which "tends to suppress differences of birth, status, profession and wealth" (Mannheim 1929b: 155). Losing the "illusion that there is only one way of thinking" (Mannheim 1936a: 12), they formed an 'interstitial stratum', with "a maximum opportunity to test and employ the socially available vistas and to experience their inconsistencies" (Mannheim 1932b: 106). By working in either a party or in academia, they could then fulfil "their mission as the predestined advocate of the intellectual interests of the whole" (Mannheim 1929b: 158).

Further, if intellectuals were uniquely placed to achieve a dynamic synthesis of political knowledge and practice, sociologists were uniquely placed among them. Each social science was useful in cultural analysis, but since only sociologists aimed at "a complete theory of the totality of the social process" (Mannheim 1936b: 203), theirs was

the basic discipline. Only through sociology could the intellectuals achieve the self-awareness they required, and it was in sociology that their dynamic and synthetic mediation could be focused. The legislative strain in Mannheim's work, latent in his early and uneasy exemption of scientific knowledge from existential determination, became manifest in this metadisciplinary ambition. Relatedly, he accentuated after his move to England the quasi-religious terms in which he had defended the sociology of knowledge.¹⁷ Given the rise of fascism, the Depression, and impending war, the risks of social dissolution were such that "the successful organization of society cannot be left to chance" (Mannheim 1935: 7). The intellectuals had to save liberal democracy from its excesses. In the restoration of a (Durkheimian) spiritual sense to the body politic, society would have to be planned, and "[t]here will ... in every planned society be a body somehow similar to the priests, whose task it will be to watch that certain basic standards are established and maintained" (Mannheim 1943: 119). Adopting such 'social techniques' used under fascism as propaganda or behaviourist reconditioning, "groups with determined political will, equipped with the necessary psychological and sociological knowledge" (Mannheim 1937: 300) should educate the masses in their potential and duties.

In that version of a scientifically warranted group mind directing the group soul, Mannheim had returned to his point of departure from Scheler. He too now looked upon the world with God's eyes. The sociology of knowledge was now a legislative metadiscipline, not just for sociology but for the social world at large.

Sociology of knowledge: Reprise

That prescriptive strain in the sub-/metadiscipline matched its political origins. Scheler had been engaged throughout the period,¹⁸ while Mannheim, twice an exile, bracketted his years in Germany with activism. Both assumed that authoritative

¹⁷ "We shall have to transform the sociology of function ever more into a sociology of mission" (from a letter Mannheim wrote to his students in 1930, quoted in Kettler and Meja 1995: 122)

¹⁸ Through World War I, Scheler was "*the* war philosopher" (Becker 1943: 208). Under Weimar, "[t]he inner elective affinity of the versatile, adaptable, rather spineless Scheler with the ideological needs of this short transitional period raised him for a while to the authoritative thinker of bourgeois Germany" (Lukacs 1952: 488-9).

knowledge was required to resolve social disorder, both proposed that their sociologies of knowledge fulfilled that need, and both used the same rhetorical moves in response to the circularities in their assumptions that "[a]ll human knowledge ... is not empirical but 'a priori' knowledge" (Scheler 1925: 67) or that "strictly speaking it is incorrect to say that the individual thinks" (Mannheim 1936a: 3). Scheler to some extent, as in his version of Comte's stages, and Mannheim far more elaborately, used trinary interaction in allowing for the effects of existential determination. Scheler from the start, in separating world from spirit, real from ideal factors, or group soul from group mind, and Mannheim increasingly, used binaries to foreclose on that instability and to reclaim an Archimedean point which would be both analytically and politically authoritative. The legislative strain is given in the rhetoric.

That in turn entailed a tacit denial of rhetoric, in the omission of attention to language. While both accounts of knowledge were effects of reading and writing - few strands of the emergent sociological tradition, or, indeed, of the western tradition more generally, cannot be found in them - those processes could not easily be accommodated. Scheler excluded the act of reading from his schema: if language is symbolic, and if the phenomenological *epoche* consists of the "continual *desymbolization of the world*" (Scheler 1913-4: 143), then he eliminated a potential mediation between group soul and group mind. Mannheim's case is more complex; since the essayistic 'total perspective' of his 'experimental thinking' suggests a literary cubism, he can hardly be accused of ignoring language. But, as Mills (1939) noted early, he never used linguistic variation to link social action with social location.¹⁹ If only by default, the sociology of knowledge in its metadisciplinary moment then rested on the neutral metalanguage implied in the positivism of Scheler's and Mannheim's critiques; it was a rhetoric of anti-rhetoric.

Even as a modest subdiscipline, the sociology of knowledge was ambivalently received in the discipline at large. Scheler was not widely read, and while publication of both *Ideologie und Utopie* and its translation were disciplinary events (Nelson 1990;

¹⁹ He did so in his own practice, if not his analysis. In discussing Mannheim's manoeuvres over the reception of his work, Kettler and Meja (1995: 248) mention his "rhetorical sensibilities" and his implicit use of the "classical distinction between sophist and philosopher". It is characteristic of the field, too, that despite his noting of Mannheim's omission and despite his own call for the semiotic analysis of knowledge, Mills himself scarcely linked language and social location.

Kettler and Meja 1995), few sociologists adopted Mannheim's 'schema of orientation'. When the 'science' which had so uncertain a place in it was subjected to sociological analysis, that study was linked only tenuously to Scheler's and Mannheim's work. But, as seen in Merton's and Kuhn's accounts of science, its rhetorical structuring was similar.

SOCIOLOGY OF SCIENCE

The sociology of science is commonly dated from Merton's (1938a) thesis on the Puritan ethic and the spirit of science. While Merton himself acknowledged Hessen (1931) and other precursors, it was he who located the study of science as an institution within an emergent central movement in sociology. Yet despite its place in structural-functionalism, this subdiscipline was for a time no more successful than the sociology of knowledge to which it was loosely articulated; it did not flourish until after Kuhn published his *Structure of Scientific Revolutions* a quarter of a century later. Apart from this repetition of limited adoption, the sociology of science is characterised by the same tension between the epistemological and political senses of representation and by the same strain towards dualism as found in Scheler's and Mannheim's work.

Merton

Like Mannheim, Merton has favoured the essay, and as with Mannheim, attention here is concentrated on his early papers. But where a discontinuity was stressed between Mannheim's early sociology of knowledge and its limiting in his later work, Merton's *oeuvre* shows a remarkable consistency over his remarkable career; the prose becomes more elaborate, even rococo, but from his earliest papers to his latest the content and form of his arguments remain the same. His typically antithetical style is shown here in his sociology of knowledge, his distinction between 'culture' and 'civilisation', and his account of 'the norms of science'. That characteristic style expresses the characteristic problems of the sub-/metadiscipline, and through Merton's disciplinary standing it suggests as well their recurrence in sociology at large.

Where Scheler had located the sociology of knowledge within a critique of

epistemology, and where Mannheim had opposed the two, Merton avoided the issue. After granting that “the perennial problem of the implications of existential influences upon knowledge for the epistemological status of that knowledge has been, from the very outset, hotly debated” (Merton 1945: 372), he simply assumed the analytical priority of epistemology. In reviewing Znaniecki’s (1940) sociology of knowledge, for example, Merton (1941b: 112) praised the way Znaniecki had “not confused problems in [it] with a sociological theory of knowledge, that is, with a social epistemology”.²⁰ Relativism was the problem. Reading Scheler’s Pascalian phenomenology as “counter-relativism by fiat”, Merton (1945: 385; 1937: 495-6) rejected his “extreme historicism”.²¹ As for Mannheim, on the usual grounds of vicious circularity, Merton (1941a: 557, 561, 562) described his work as flawed by “logical fallacy and intellectual nihilism”, as inducing “intellectual anarchy”, and as leaving a critical question unanswerable: “[o]nce given the existential determination of thought, who is there to judge among the babel of competing voices?”. The answer is implied. Merton began where Mannheim ended, in logically purified authority; an epistemological guarantee was one part of the solution to the structural-functional problem of order.

Merton made the same move in his reading of Alfred Weber’s and Robert MacIver’s distinction between ‘civilisation’ and ‘culture’. Like Mannheim, he used it to contrast the impersonal/objective with the personal/subjective. The replicability of a chemical experiment differed from the impossibility of Shakespeare’s work being duplicated; “[i]t is this basic difference between the two fields which accounts for the cumulative nature of civilization and the unique (noncumulative) character of culture” (Merton 1936a: 112). The circularity of studied knowledge is given in the distinction, for Merton (1936a: 111) defined science as civilisational, and civilisation by the scientific method: “[i]t is a peculiarity of civilizational activities that a set of operations can be so specifically defined that the criteria of the attainment of the various ends are clearly evident”. But where Mannheim added an interactive third term - ‘dialectical progress’ - to allow for that circularity, Merton let the binary of civilisation and culture stand. In

²⁰ Znaniecki’s (1940) sociology of knowledge could be described as a Bergsonian and interactionist functionalism.

²¹ Scheler’s own rejection of historicism might be recalled.

various forms it is a permanent feature of his work.

It recurs, for example, in Merton's (1942: 268-9) account of the 'ethos of science', that "affectively toned complex of values and norms which is held to be binding on the man of science". Such culturally legitimated norms were said to shape the consciences of scientists in achievement of their civilisationally defined institutional goal, extension of those "empirically confirmed and logically consistent statements of regularities" (Merton 1942: 270) comprising knowledge. Merton suggested that the ethos entailed four norms: universalism, communism, disinterestedness, and organized scepticism.

Using Parsons' distinction between universalism and particularism, Merton (1942: 270) held that truth-claims in science were judged by "*preestablished impersonal criteria*". Since such particularistic considerations as the nationality of the scientist were precluded, science was institutionally distinctive: what might be patriotic in other contexts was condemned in it. But universalism still had political implications, in the requirement that science be open to talent. Insofar as democratisation implied progressive elimination of restraints on socially valued capacities, "the ethos of democracy includes universalism as a dominant guiding principle" (Merton 1942: 273); science and a particular form of democracy were mutually affirmative. Secondly, since science was a communal process, involving collaboration not only among contemporaries but between each of them and the heritage of their fields as well, scientific findings were not 'owned' by their proponents but were assigned to the scientific community at large. With knowledge the result of "competitive cooperation" (Merton 1942: 273-4),²² a scientist's only 'property right' was such recognition as accrued to a finding, not the finding itself. Thirdly, treating 'disinterestedness' through Parsons' (1939) analysis of the professions and through Marxist studies of 'interests', Merton distinguished between institutional and motivational levels of analysis. Whether individual scientists were altruistic or self-interested, science as a whole ensured detachment, with all claims treated equally. Finally, as both an institutional and a methodological mandate, 'organized scepticism' implied "temporary suspension of judgment and the detached

²² Merton did not include among his characteristically wide-ranging references any mention of Mannheim's (1928a) Hegelian reading of Heidegger's 'public interpretation of reality', where competition was treated as a determinant in intellectual life.

scrutiny of beliefs in terms of empirical and logical criteria" (Merton 1942: 277); the technical was to be quarantined from the social.

That separation of cognitive/civilisational from social/cultural norms is the basis of Mertonian sociology of science,²³ and of the 'middle-range' scientificity Merton did so much to promote within sociology in general. In avoiding 'Mannheim's paradox' by the assumption of epistemological priority, in specifying *the* norms of science, and in linking the invisible hand of 'competitive cooperation' with democratic social order, he set a metadisciplinary programme which was far more widely adopted than his subdisciplinary work. But the 'dialectical progress' which Mannheim introduced between 'civilisation' and 'culture' still appears in Merton's work, as lacunae. Berger and Luckmann (1966: 23), for example, noted as an oddity in his sociology of knowledge that he did not reconcile it with his work on reference groups, and the same could be said of his echoing of Wirth's (1936) point, that Mannheim's treatment of existential determination was consonant with American pragmatism (Merton 1941a: 562). Had he elaborated on the interactively social and technical in those, his epistemological givens could not have stood.

So Merton reached the same point as had Scheler and Mannheim, and through similar rhetorical foreclosures. Nor was he any more successful than they had been in translating metadisciplinary legislation into subdisciplinary research. The sociology of science in his early work would not flourish until Kuhn's work was assimilated to it.

Kuhn

Where Merton had judged sociology epistemologically, the scandal of Kuhn's early work lay in his contention that scientific rationality was itself emergent. Like Scheler, Kuhn treated science as if generated by a 'group mind', like Mannheim, he avoided antinomy, by positing an 'essential tension' between tradition and innovation (Kuhn 1959), and, like

²³ Gieryn, Merton's former student and collaborator, exemplifies disputes over 'the norms of science'. In one paper he attacked "the pseudo-novelty of the relativist/constructivist program by comparing it to a most familiar antecedent, the theoretical orientation developed by Robert Merton" (Gieryn 1982: 280). He has since adopted the constructivist view, that scientific knowledge is "a contextually contingent and interests-driven pragmatic accomplishment drawing selectively on inconsistent and ambiguous attributes", and that Merton had imposed essentialist boundaries on science instead of tracing how they were achieved (Gieryn 1995: 393).

Merton, he focused on the collective practices of scientists. But he also brought a new element to the study of knowledge, a Wittgensteinian attention to language. Again like Mannheim, however, Kuhn did not sustain his 'essential tension'; against his early opening of the rational, he ended in reaffirming epistemology over the arational and sociological.

Both the circularity of any study of knowledge and the beginning of Kuhn's return to dualism are given in the terms of his 'essential tension'. He equated 'tradition' with 'normal science' and 'innovation' with 'scientific revolutions'; the former denoted work within, and the latter a shift of, 'paradigm', where "a paradigm is what the members of scientific community share, and, conversely, a scientific community consists of men who share a paradigm" (Kuhn 1970b: 176). Intra-paradigmatic work, no less engaging for being in a sense routine, occupied most scientists most of the time. Since theories were always underdetermined by evidence,²⁴ 'normal science' consisted of the filling of empirical gaps and of attempts to resolve any anomalies implied by them. This was less a discovery of nature than "an attempt to force nature into the preformed and relatively flexible box that the paradigm supplies" (Kuhn 1970a: 24). On this view of science as puzzle-solving, theories such as verificationism or Popper's hypothetico-deduction were inadequate, for it was historically evident that scientists did not follow individually pioneering cycles of conjecture and test.

Inevitably, some puzzles in a paradigm remained. Accumulation of anomalous findings or unsolvable problems led to "a period of pronounced professional insecurity" (Kuhn 1970a: 67), or 'crisis', which might end in a 'scientific revolution' and the advent of a new paradigm. Since this would not be conclusive, "the act of judgement that leads scientists to reject a previously accepted theory is always based upon more than a comparison of that theory with the world" (Kuhn 1970a: 94). It was in part arational, and argument between adherents of old and new paradigms might then be limited. As in a gestalt switch, the new normal science may be "actually incommensurable with that which has gone before" (Kuhn 1970a: 103). This was a linguistic process. Changes in the

²⁴ On the Duhem-Quine hypothesis. Comte (1830-42 I: 3) too had stressed the theory-ladenness of sociological inquiry: "All good intellects have repeated since Bacon's time, that there can be no real knowledge but that which is based on observed facts ... it is equally true that facts cannot be observed without the guidance of some theory".

meanings of terms may lead to workers in different paradigms being mutually incomprehensible, especially as “the competition between paradigms is not the sort of battle that can be resolved by proofs” (Kuhn 1970a: 148). Such battles were always both incomplete and collective. Assuming different criteria of importance and plausibility, “the proponents of competing paradigms practise their trades in different worlds” (Kuhn 1970a: 150). The adoption of one rather than another “proves to be a choice between incompatible modes of community life” (Kuhn 1970a: 47). This had to be so, since “scientific knowledge, like language, is intrinsically the common property of a group or else nothing at all” (Kuhn 1970b: 210).

Since this account of science relied on both exemplars and language, the word ‘paradigm’ was appropriate, but its imprecision induced widespread critique.²⁵ Accepting the point, Kuhn developed an alternative. Once a scientific community had been identified it could be asked “What do its members share that accounts for the relative fulness of their professional communication and the relative unanimity of their professional judgments?” (Kuhn 1970b: 182). He proposed that ‘disciplinary matrix’ replace ‘paradigm’. It was here that he echoed Mannheim, for rather than the either/or of paradigms the ‘disciplinary matrix’ was trinary. Like Mannheim’s fusion of science, historicism, and phenomenology, it consisted of ‘symbolic generalisations’, ‘models’, and ‘exemplars’. Denoting “those expressions, deployed without question by the group, which can readily be cast in some logical form” (Kuhn 1974: 297), ‘symbolic generalisations’ had a dual effect; an equation such as $f=ma$ both recursively defined its elements and entailed commitment to logico-mathematical argument. As in the effects of light being taken as either waves or particles, ‘models’ “provide the group with preferred analogies or, when deeply held, with an ontology” (Kuhn 1974: 297-8). ‘Exemplars’, finally, were lived, being “concrete problem solutions, accepted by the group as, in a quite usual sense, paradigmatic” (Kuhn 1974: 298).

The ‘disciplinary matrix’ is then *itself* an exemplar of a characteristic move in the study of knowledge, and even if the interaction between its three moments is not quite symmetrical, the asymmetry ends in openness rather than foreclosure. Although

²⁵ Masterman’s (1970) noting of twenty-two separate senses in which Kuhn used ‘paradigm’ must be the most cited work in the extensive commentary on *Structure*.

scientific change entails change in all three, one is logically and experientially prior. Since any number of sciences may share symbolic generalisations, a given discipline or subdiscipline is defined less by them than by models and exemplars. But models are to a degree predisciplinary, serving in their ontological sense as attractors *to* a discipline rather than as indicators *of* it; for the student in a field, acquisition of its symbolic generalisations and lived familiarity with exemplars actuating their meaning are the critical processes (Kuhn 1974: 307). At the junction of those two distinctions, exemplars are the core of the disciplinary matrix. Exposure to their illustrative and channelling effects imbues the student with that “acquired ability to see resemblances between apparently disparate problems” (Kuhn 1974: 306) which comprises much of science. In this sense, exemplars should be taken as operating analogically or metaphorically, but more crucially as lived.²⁶ That is what gives the disciplinary matrix its openness. Exemplars link the rational and the arational. Within the ‘semantic fields’ of ‘disciplinary clusters’ (Kuhn 1983: 567), they both identify disciplines and allow analytical access to them. Such access, Kuhn claimed, is sociological.

That was crucial to his work from the start: his early reading of Fleck (1935) confirmed that his problematics “had a fundamentally sociological dimension” (Kuhn 1979b: viii); his observation of differences between natural and social scientists over “the nature of legitimate scientific problems and methods” had helped crystallise his sense of ‘paradigm’ (Kuhn 1970a: vii-viii); his account of the ‘scientific community’ was “intrinsically sociological” (Kuhn 1970c: 238). But against that set of claims, it is striking how unfamiliar with sociology Kuhn appeared to be.

Granted, the sociology of science was little developed when he published *Structure*. But even if the subdiscipline was “not now one of the most highly cultivated areas in sociology”, its problems were held to be “primarily practical ... problems of personnel rather than theory” (Barber 1959: 227, 228). Given his disciplinary claims, and given that he had been Barber’s fellow student and colleague at Harvard, it is all the more odd that Kuhn scarcely referred in *Structure* to the sociology of science. When he

²⁶ cf. Durkheim (1893: 362-3): “Each science has, so to speak, a soul which lives in the conscience of scholars. Only a part of this assumes sensible bodily form. The formulas which express it, being general, are easily transmitted. But such is not the case with the other part of science which no symbol translates without. Here, all is personal and must be acquired through personal experience”.

mentioned the discipline, his referent seems to have been sociology in the abstract, or sociology as a synonym for the social, rather than sociology as practised; it appears a residual, available to anyone, not in itself a disciplined way of knowing. Kuhn (1970c: 235) made this explicit in replying to Popper's (1970: 58) claim that it was 'surprising' and 'disappointing' that he should turn for his rational reconstruction of science to sociology or psychology :

If he means that the generalizations which constitute received theories in sociology and psychology (and history ?) are weak reeds from which to weave a philosophy of science, I could not agree more heartily. My work relies on them no more than his.

Kuhn never elaborated this scant endorsement of what was supposedly central. Oddly for an exponent of the 'disciplinary matrix', his writing is extradisciplinary; as Fuller (1992: 275) said of *Structure*, it had "a philosopher's sense of sociology, a historian's sense of philosophy, and a sociologists' sense of history". But if he did not engage the sociological debates, he continued to insist on his sociological grounding. In terms which recall Scheler and Mannheim, he claimed in one re-justification that:

Traditional discussions of scientific method have sought a set of rules that would permit any *individual* who followed them to produce sound knowledge. I have tried to insist, instead, that though science is practiced by individuals, scientific knowledge is intrinsically a *group* product and that neither its peculiar efficacy nor the manner in which it develops will be understood without reference to the special nature of the groups that produce it. In this sense, my work has been deeply sociological, but not in a way that permits the subject to be separated from epistemology (Kuhn 1977: xx).

That is the critical divide. On the one hand, it is programmatically sociological; Kuhn (1974: 295) welcomed the studies of 'scientific communities' by such sociologists as Price, Crane, Hagstrom and Mullins, and defended Merton's 'ethos of science' (Kuhn 1977: xxi). But, on the other, epistemology remains. Just as he denied Feyerabend's (1970) reading of his work - Kuhn (1970c: 263-5) was only relatively a relativist - he disowned those who took epistemology itself as intrinsically social (e.g. Kuhn 1977: xxi). His last study (Kuhn 1978) was conventionally internalist; since no Kuhnian terms appear in it, such as 'paradigm', 'puzzle-solving', or 'disciplinary matrix', it has been seen as "the final stage of a process of retraction initiated by Kuhn in response to some of the reactions which *Structure* produced" (Pinch 1979: 440).

So, through disavowal of his own already tenuously held sociological moment

Kuhn ended at the point where Mannheim also ended and where Merton began: ²⁷ rules of rationality preceded sociological inclusion of the arational; civilisation subsumed culture; generalisations were more certain than lived exemplars.

Sociology of science: Reprise

In Merton's 'civilisation' and 'culture' and in Kuhn's self-disrupted disruption of dualisms, the metadisciplinary moment in the sociology of knowledge is stressed over the subdisciplinary. Once the epistemological was separated from institutionalised normativity or communal messiness, the ordering of science became a matter of social order. Against Mannheim's 'intellectual nihilism' or 'anarchy', Merton identified science with liberal democracy, and the same move appears in Kuhn's controversy with the Popperians. Ryan (1972: 233), for example, held that Kuhn's account of science "has been a paradigm not of the life of the open society, but of a totalitarian community", and Kuhn's later stress on epistemology implied a concession to the rational authority he claimed. But if Merton and Kuhn then repeated much of Scheler and Mannheim, and if their epistemologically warranted knowledge required a neutral meta-language, language had still been problematised in their sociology of science in a way it had not been in the sociology of knowledge.

In Merton's case, the problem emerges as a contradiction. In another expression of his distinction between civilisation and culture, he held that scientific sociology was independent of the language in which it was presented:

... the closer [a sociologist] approaches eloquence, the further he retreats from methodical sense. ...

... ostensibly scientific reports often become obscured by irrelevancies. In extreme cases, the hard skeleton of fact, inference and theoretical conclusion become overlaid with the soft flesh of stylistic ornamentation ... (Merton 1968b: 69-70).

This oddity is typical, for despite his distancing of 'eloquence' from 'methodical sense',

²⁷ Merton, whom Fuller (1992: 242) has called "the most artful 'Kuhnnotropist'", has repeatedly stressed this. See especially Merton (1977).

Merton has a reputation as an effective stylist.²⁸ So on his practice rather than on his pronouncements, his work is a Kuhnian exemplar; if sociology is a science, sociological knowledge is a literary effect. Kuhn, of course, made that explicit, for whatever his shifts in conclusions, he kept a stress on the linguistic character of scientific knowledge.

That emphasis is a crucial move in the study of disciplined knowledge. Merton may have operated through omission and contradiction, Kuhn may have retracted what others have found most suggestive in his work, and both may have retained the purifying distinction between epistemological and political representations, but each presented exemplary means of opening the preemptions of Scheler's and Mannheim's sociologies of knowledge. Those means have been followed, in form and in content, in the sociologies of scientific knowledge derived in part from their work.

SOCIOLOGIES OF SCIENTIFIC KNOWLEDGE

Sociologies of scientific knowledge have been directed to foreclosures in the sociologies of knowledge and science. Where Scheler, Mannheim, Merton and Kuhn had either begun or ended in exempting scientific knowledge from existential determination, writers here take it as axiomatic that the products of science are as 'social' as the contexts of their production. But a reading of the sociologies of scientific knowledge as continuous with the sociologies of knowledge and science is tendentious. When Scheler and Mannheim first wrote there were no sociologists of knowledge, the same is true of Merton and sociologists of science, and when Kuhn published *Structure* there were few practitioners in either (sub)discipline. Each of the four located his writing within and beyond sociology. Now, after an exponential increase in the number of those identified with the study of knowledge and science, such simultaneous recursion and dispersion is more pronounced. Ben-David (1981) noted that some sociologists of scientific knowledge appeared unfamiliar with sociology proper, and those within the field have likewise

²⁸ e.g. "Merton's writing is a legend in sociology because it is literary in the best sense" (Sica 1988: 137). Or, his "methodological stance carried weight not to much because it was original but because of its careful, detailed and well-written presentation" (Crothers 1987: 52). In a nice reversal, Stein (1963: 177) used the argument of self-refutation: "Merton's defence of neutral language defeats itself by the partisan vigor with which he repudiates opposing conceptions".

stressed the variety of their sources.²⁹ But despite the disparate allies enrolled in challenges to the 'traditional' sociology of knowledge, to Merton's model of science, and to the preemptive boundaries of Kuhn's 'scientific community', there is a common focus on the practical, communicative, and embodied processes by which knowledge becomes authoritative.³⁰ Despite, too, a conventional distancing from earlier work, familiar moves recur in the sociologies of scientific knowledge; indeed, they are evident in the very contrast. The appearance of these difficulties in the act of their questioning will be illustrated here through two bodies of work: Bloor's and Barnes' 'strong programme'; and Mulkay's 'discourse analysis'.

The strong programme

As an obligatory point of passage in sociologies of scientific knowledge, the 'strong programme' is itself documentary of Kuhn's essential tension between tradition and innovation. Most of the issues raised throughout this chapter are addressed in it. Refusing the epistemological and the political, for example, Bloor (1976: 65) looked to "an isomorphism between a tradition of ideological dispute and an epistemological debate". Where the distinction between civilisation and culture has been read as symptomatic of those debates, Bloor (1976: 144) collapsed it, in holding that whatever practices undergird the sciences, "that foundation is our culture. Science is our form of knowledge". Where much sociology of knowledge has entailed a purifying rejection of relativism, Barnes and Bloor (e.g. 1982) have polemicised for it. Where language had been ignored, Bloor and Barnes followed Kuhn in using Wittgenstein's language-games to show that a sociology of scientific knowledge must be linguistic; Bloor (1973: 173) had earlier invoked Wittgenstein to "solve...Mannheim's problem", on the exemption of mathematics from

existential determination, and in taking 'concrete puzzle solutions' as the key to Kuhn's

²⁹ Collins (e.g. 1983; 1996) has emphasised this. The 'Here and Everywhere' with which Shapin (1995) titled his review of the field likewise refer as much to its sources as to its applicability. See also Lynch and Bogen (1997). Both Shapin and Lynch and Bogen have also stressed that acquisition of the scientific expertise so useful for a sociology of scientific knowledge is not easily combined with sociological training.

³⁰ Whether Mertonian/Kuhnian sociology of science and the sociology of scientific knowledge are complements or alternatives is itself moot (e.g. Collins 1983; Zuckerman 1988). Knorr-Cetina's (1982) account of 'transepistemic arenas' is one paradigmatic reading of Merton and Kuhn. Gieryn (1995) and Shapin (1995) exemplify the oppositional casting of the field.

'paradigm', Barnes (1982) likewise stressed their relation to Wittgenstein's learned similarities and forms of life. The field could be described by typical language games, as when Bloor (1976: 74) recalled an early binary:

The antithesis between the Benthamite lust for 'codification' and clarity and Burke's claims about the the role of prejudice corresponds to the difference between Popper's methodological legislation and boundary drawing and Kuhn's stress on dogma, tradition and judgment.

If the strong programme was an innovation it was through insistence on the tradition

Even the directions in it were not claimed as new, but were "an amalgam of the more optimistic and scientific strains to be found in Durkheim, Mannheim and Znaniecki" (Bloor 1976: 5). Mention of Durkheim is salient, for where Mannheim was shown as generalising from Marx, and Merton from Weber, the 'strong programme' is a reprise of collective representations. While Durkheim's "hints [may] have fallen on deaf ears", his and Mauss' account of classifications "went to the heart of the matter" (Bloor 1976: 4; 1982: 267); Bloor (1983) depicted his Wittgensteinian 'social theory of knowledge' as a redressing of Durkheim's own breach of his theory.³¹ All that is a paradigmatic case of use of the tradition to question the tradition, or of treatment of "knowledge [as] a resource in activity and not a determinant of it" (Barnes 1977: 85).

The Durkheimian sense is clear in Bloor's (1976: 2) assumption that sociologists are "concerned with knowledge, including scientific knowledge, purely as a natural phenomenon". He included four points in his manifesto for sociological treatment of the social facts of knowledge as things. It would be: *causal*, being addressed to conditions conducive to beliefs or states of knowledge; *impartial* over rationality or irrationality, success or failure, truth or falsity (the self-evident character of scientific knowledge could not be assumed, as Mannheim and Merton had); *symmetrical* in explanation (successful and failed claims to knowledge were to be explained in the same terms); and *reflexive*, being in principle applicable to sociology itself (Bloor 1976: 4-5). This strong

³¹ "For Durkheim a scientific explanation might be found for primitive systems of classification, but the same could hardly apply to the respected achievements of our own, scientific culture. In the early stages of cultural evolution, he said, a belief may be deemed true because it is socially accepted; but for us it is only socially acceptable on condition that it is true. By succumbing to this seductive idea Durkheim allowed himself to throw away all he had so painfully won" (Bloor 1983: 3). In Barnes' work, there is less stress on Durkheim and more on Marx, but the same sense of use of a tradition.

programme subsumes much of the work treated in this chapter. The insistence on causality is in effect a revisiting of Scheler's metaphor of real factors acting as a sluice-gate on the spiritual/cultural stream; the calls for impartiality and symmetry entail study of scientific knowledge as achievement in a way which extends Mannheim's 'dynamic synthesis' and lifts Merton's dualistic preemptions; impartiality allows for Kuhn's sense of emergent rationality; and the stress on reflexivity makes explicit the simultaneously sub- and metadisciplinary character of the study of knowledge. In the inclusiveness of both these methodological principles and their genealogical grounding, the strong programme is itself a collective representation of collective representations.

But a doubt remains over this approach to recurrent difficulties. Despite Bloor's noting of the antithetical structuring of debates, despite his dual attention to ideological disputes and epistemological issues, and despite his collapsing of the distinction between 'civilisation' and 'culture', he presumes what he questioned in his principle of causality. A demonstration of causality requires that independent and dependent variables be drawn from knowledge and its context, and such neo-Schelerian 'real' and 'ideal' factors imply as well the Schelerian ontologies of spirit and world; they imply, that is, a return of the distinction of 'nature' from 'culture'. It is curious, too, that when Bloor (1991) responded to his critics, his audience were philosophers rather than sociologists. Since his work, like Kuhn's, is programmatically sociological but practically epistemological he has reenacted the preemptions of his problematic.

A similar effect is found in Mulkay's more empirical discourse analysis.

Discourse analysis

Mulkay too has treated scientific knowledge through its language, and so has lifted foreclosures in the sociologies of knowledge and science. But he too has blocked his openings by repeating the structure of those sociologies; the 'strain towards dualism' characteristic of the field recurs throughout his work. The effect will be shown here in three phases, his uses of discourse analysis, conversation analysis and rhetoric.

Using 'discourse' as "a convenient way of referring to all forms of verbalization: to all kinds of talk and to all kinds of written documents" (Mulkay 1981: 169-70), Mulkay

and his colleagues (mostly black-boxed under his name) elaborated its analysis in response to perceived weaknesses in quantitative and qualitative methods commonly used in the sociologies of science and of scientific knowledge: on the one hand, citation-analysis did not allow for "the interpretative work embodied in scientists' discourse" (Mulkay *et al.* 1983: 193), while, on the other, ethnographers often missed the occasionality of scientists' inconsistency. Discourse analysis was a solution:

the analyst cannot move from the highly variable appearances constructed in the course of scientists' discourse to a specification of the connections between 'real actions', without first understanding how that discourse is flexibly organized to produce divergent versions of action... we must turn our attention, at least initially, from the nature of scientific action (and belief) to the systematic procedures by which scientists construct the various versions of their actions (and beliefs) in the course of interaction (Mulkay *et al.* 1983: 193).

While treatment of scientists' discourse as topic rather than as resource meant that any hope of definitive accounts had to be abandoned, Mulkay did claim that this method gave a surer basis for what could be said. Through attention to the socially generated character of its topic, discourse analysis was truly sociological in a way which neither citation analysis nor ethnographic study were. It might even allow positive scientific study, as "the analyst is no longer required to go beyond the data" (Mulkay and Gilbert 1982: 314). It might not replace other methods, but it was at least "a necessary prelude to the satisfactory resolution of traditional questions" (Mulkay and Gilbert 1982: 315). As a kind of 'natural history of social accounting', it was "the only approach which holds out a promise, by its very nature, of rebuilding the sociological analysis of action and belief on a firmer methodological basis" (Mulkay *et al.* 1983: 199).

Mulkay constructed much of his discourse analysis through the 'interpretative repertoire'. Deriving this "lexicon or register of terms and metaphors drawn upon to characterise and evaluate actions and events" (Potter and Wetherell 1987: 138) from Halliday's (1978) 'linguistic register', he used it to describe scientists' generation of the meaning of 'science' in formal and informal contexts. In their exemplary study of the language used by biochemists in formal papers and in interviews, Gilbert and Mulkay (1984: 40) claimed to show that:

when scientists write experimental research papers, they make their results meaningful by linking them to explicit accounts of social action and belief; that the accounts of action and belief presented in the formal research literature employ only one of the repertoires of social accounting used by scientists informally; that these formal accounts are couched in

terms of an empiricist representation of scientific representation of scientific action; that this empiricist repertoire exists alongside an alternative interpretative resource, which we have called the contingent repertoire; that this latter repertoire tends to be excluded from the realm of formal discourse; and that the existence of these formally incompatible repertoires helps us to begin to understand the recurrent appearance of interpretative inconsistency in scientists' discourse.

Through the empiricist repertoire scientists achieved that conventional impersonality by which they depicted the natural world as speaking for itself and themselves as both compelled by it and constrained by the rules of experimental procedure. The 'repertoire' comprised "certain recurrent stylistic, grammatical and lexical features which appear to be coherently related" (Gilbert and Mulkay 1984: 55-6), and which collectively amounted to representations of 'the real'. The contingent repertoire was differently coherent. Scientists used it to depict others' actions as "the activities and judgements of specific individuals acting on the basis of their personal inclinations and particular social positions" (Gilbert and Mulkay 1984: 57), rather than as generic responses to nature.

The two repertoires then allowed an ordering of scientists' accounts of their work. Mulkay (1984: 532) evoked a similar sense of "organized solutions to the ... task of reconciling two formally contradictory sets of interpretative constraints" when he treated 'accounts' ethnomethodologically, in enrolling work on conversational responses to praise to analyse the ceremonies surrounding the Nobel Prize.

Pomerantz (1979) had suggested that while participants in a conversation expected mutual agreement over evaluations, compliments caused problems; the self-praise implied by assent would breach other expectations. She found two negative and two positive solutions. 'Disagreement' moderated self-praise, but violated normative agreement. 'Downgrades' - restricted acceptance of praise - were commoner, but still risked disagreement. More positively, the recipient of praise might accept it but then 'return' a compliment to the original speaker, and so both avoid disagreement and reestablish parity. Finally, the recipient of a compliment might accept the full force of a speaker's evaluation by 'reassigning' praise to a third party or parties. Again, agreement was sustained and excessive self-praise avoided.

Generalising to the 'ultimate compliment' of the Nobel prize, Mulkay read the

published texts of *Le Prix Nobel* for 1978-81 for compliments and for responses to them. His hypotheses on laureates' responses to "concerted and intense expression of praise and congratulation" (Mulkay 1984: 537) were supported. They avoided disagreement, but downgraded aspects of their work, returned compliments to Sweden, Norway and the Nobel Committee, and reassigned credit widely. Just as Pomerantz found responses to compliments to be a constitutive feature of conversation, Mulkay suggested that the laureates' reassignment of credit reflected and enacted the cohesiveness of scientists at large. While constitutivity qualified any reading of this solidarity as Durkheimian externality, the constraint found in conversation analysis and in this extension of it did result in a neo-Durkheimian account of the structural effects of ceremonies. Mulkay (1984: 548) concluded that:

high levels of praise directed at a select few participants, followed by restrained agreement on the part of the recipients and intense and widespread reassignment of credit may well be constitutive of formal celebrations of achievement in 'our culture'.

So the ceremonial face of science was structured by the same occasioned resolution of contradictory requirements as found in scientists' day-to-day discourse. That move could be generalised further, to the depictions of science in political debates, as in Mulkay's more recent studies of parliamentary debates on research on human embryos.

His analysis here turns on a distinction between a 'rhetoric of hope', which casts this specific research and science in general in as positive a light as possible, and a negative 'rhetoric of fear'. The two are not quite symmetrical:

The rhetoric of hope is an institutionalised interpretative form which is widely used in our culture to express support for current developments in science and technology. Use of the rhetoric of fear, in contrast, seems to become appropriate only when science and technology can be represented as violating basic cultural categories and moral values (Mulkay 1993: 724).

Comprising a set of claims that because science has generated benefits in the past it will continue to do so, the rhetoric of hope entails culturally embedded assumptions about 'science', 'control of nature', and 'progress'. It is "part of the taken for granted, dominant discourse of science in our society" (Mulkay 1993: 736), needing no special justification under all but extraordinary conditions. The 'rhetoric of fear', "part of a culturally subordinate discourse of science" (Mulkay 1993: 736) is available when the context is out

of the ordinary. Evoking “a ‘frightening vision’ of moral decline and of socially disruptive changes brought about by a scientific community increasingly out of control” (Mulkay 1993: 728), it is drawn from cultural resources encoding suspicion of science. The same structure recurs in Mulkay’s (e.g. 1994; 1995) other papers on the debates, on the conflicts between science and religion and science and the family; each issue is depicted in terms of contradictory and exigently available resources.

The repetition across scale of situated resolution of contradictory requirements - in scientists’ daily practice, in the celebration of scientific achievement, in political debates over science - is a version of the dynamic Mannheim posited between ‘culture’ and ‘civilisation’. Rather than separating them analytically, Mulkay treated them as generated emergently from the same resources. But if he thus developed the openness of the sociology of knowledge, he also repeated its foreclosures, for the argument and the structuring of his discursive and rhetorical analyses entail the dualistic strain towards the foundational found from Scheler’s and Mannheim’s work onwards. Like sociology at large, like science, like democracy, discourse analysis was to be an emergence from feudalism, a means of ‘ending intellectual vassalage’. It was to enable ‘the ultimate question’ to be asked, ‘basic methodological faults’ to be remedied, a ‘methodological priority’ to be established, and “fundamental and unresolved methodological issues which pervade the sociology of science, as well, perhaps, as other areas of sociological inquiry” (Mulkay *et al.* 1983: 195) to be resolved. It was presented, in short, as a means of attaining fixity. Even if its “guiding aim ... is not to provide definitive accounts or explanations of social action and belief in science” (Mulkay and Gilbert 1982: 315), it appeared to allow definitive accounts of the contingency of scientists’ accounts.³²

While Mulkay (e.g. 1985) addressed that difficulty in his deconstructive ‘new literary forms’, his typically binary analyses still entail the “evaluative asymmetry built into [use of] dual stereotype[s]” (Mulkay 1995: 500) which he has problematised. Whether or not that structure evokes the battle of good and evil he suggested through the ‘rhetoric of fear’, it does have a curious result; it reenacts Merton’s distinction of

³² Shapin (1984) drew attention to the foundational claims being made for discourse analysis.

technical from social norms of science. The location of his epistemological assumptions has been shifted, to language, but not their effects. Having departed from 'traditional' sociologies of knowledge and science, in the content of his discourse analysis, Mulkay has returned to them, in its form.³³

Sociologies of scientific knowledge: Reprise

Bloor and Barnes stressed their continuity with the tradition while Mulkay repeatedly distinguished his work from 'traditional' sociology of knowledge, but a return to old difficulties in the act of addressing them has been found in both the strong programme and discourse analysis. Bloor's and Barnes' attention to the isomorphism of ideological and epistemological disputes and Mulkay's to the situated and trans-scalar resolution of contradictory constraints both entailed an opening of the dualisms found in studies of existential determination of knowledge, but both also entailed a tacit claim to certainty at odds with an explicit stress on achievement. The strain toward dualism found first in Scheler's and Mannheim's work and then in Merton's and Kuhn's appears to be a constant in the sociologies of knowledge.³⁴

Through Mulkay's use of rhetoric, and through the Baconian optimism of his empiricist repertoire and his rhetoric of hope, this reading of those sociologies has also returned to where it began. Before an actor-network theoretical accommodation of the difficulties of the field is proposed, the consonance of the sociologies of knowledge with the rhetorical tradition will be revisited.

RETURN OF RHETORIC

Mulkay used 'rhetoric' more as a synonym for 'repertoire' than in its technical sense: it was "an interrelated set of background assumptions plus typical assertions that are

evident in participants' discourse on a particular topic" (Mulkay 1993: 723), or "a set of

³³ Mulkay (1991) dedicated his retrospective collection of papers to Merton. Similarly, Collins (1996: 232), one of Mulkay's contemporaries in the sociology of scientific knowledge, has held that "[w]hile we no longer have any reason to believe that any particular system is vital to the growth of scientific knowledge, we still know how we like our science to be done. We know that we prefer a science informed by something like the Mertonian norms".

³⁴ This will be used hereafter as a generic term for the sociologies of knowledge more narrowly, the sociologies of science, and the sociologies of scientific knowledge.

systematically used recurring textual features whereby texts gain their persuasive power" (Pinch et al. 1992: 287). But while it can stand for the general linguistic turn in the study of knowledge,³⁵ rhetoric also has specific benefits for it. It was suggested earlier that the sociologies of knowledge were genealogically linked to the rhetorical tradition. Ever since Corax and Tisias set "methodical rules for speaking" (Hinks 1944: 61), rhetoric, like the sociologies of knowledge, has entailed a rationalisation of the irrational. The tension over epistemology stressed throughout this chapter recalls debates at least as old as Plato's rejection of the epistemic scepticism of Protagoras or Gorgias,³⁶ and the consonance of recent recoveries of rhetoric with the sociologies of knowledge suggests a means whereby their recurrent foreclosures might be opened.

The crucial recognition in rhetoric is that contrary statements may be no less reasonable, one than another. Against epistemology, this requires that "the appeal to reason must be identified not as an appeal to a single truth but instead as an appeal for the adherence of an audience" (Perelman 1979: 13-14).³⁷ That emphasis defines the widely enrolled 'new rhetoric' (Perelman and Olbrechts-Tyteca 1958). Both Plato and Aristotle had developed a proto-social psychology, in relation to a rhetor's ability to sway an audience (cf. Billig 1987), but here the stress is reversed: all discourse must be adapted to what is accepted by the audience. Otherwise, an attempt to persuade would be a correlate of the question-begging of formal logic, as the rhetor would presume a consensus which does not exist and to the obtaining of which the discourse should be directed (Perelman 1979: 14-15). If contrary arguments can always be mounted, the common-sense and common values forming premises in rhetorical quasi-logic must also be inherently contradictory, or at least composed of potentially contrary elements. The

³⁵ Speech-act theory, structuralism and post-structuralism, semiotics, sociolinguistics, various forms of discourse analysis, and ethnomethodological conversation-analysis have all been used.

³⁶ Untersteiner (1948: 145) translates this summary of Gorgias' 'tragedy of knowledge': "I say that nothing exists; then that if it exists it is unknowable; lastly, even if it exists and is knowable it cannot be directly communicated to anyone else".

³⁷ Bacon (1605: 179) had made the same point: "it appeareth also that logic differeth from rhetoric, not only as the fist from the palm, the one close, the other at large; but much more in this, that logic handleth reason exact and in truth, and rhetoric handleth it as it is planted in popular opinion and manners. And therefore Aristotle doth wisely place rhetoric as between logic on the one side, and moral or civil knowledge on the other, as participating of both; for the proofs and demonstrations of logic are toward all men indifferent and the same; but the proofs and persuasions of rhetoric ought to differ according to the auditors".

rhetor may “select certain elements on which he focuses attention by endowing them, as it were, with a ‘presence’” (Perelman 1979: 17), but the selection can be contested, and the range of possible selections is limited. As a consequence, “the image of the powerful orator playing masterfully with the emotions of the helpless crowd is a myth ... It can be argued that if orators can control crowds, it is only because crowds control orators” (Billig 1987: 195). Given that persuasion is always situational, and given that what is taken for granted by an audience can provisionally be read back from appeals to it, a consonance between rhetoric and the sociologies of knowledge is evident.

The link between them was drawn early, in Speier’s (1937: 164) aside on the Sophists as “the first sociologists of knowledge in Mannheim’s sense”. Against such epistemological dismissal, Brown (1983: 127) connected the sociologies of knowledge to rhetoric as “a political phenomenology of discourse”. But where he held that they allow treatment of the same events from opposite but complementary starting points - the sociology of knowledge “examines how social forces affect belief and consciousness” while rhetoric “examines how speech affects political action” (Brown 1983: 127 n.2) - the two are taken here as co-extensive. That is given in a common stress on trinary ordering.

The move from binary analysis to triadic interaction in Mannheim’s account of epistemology as emergent is the same move as in Aristotle’s ‘inverse law of excluded middle’: since enthymemic logic depends on omission of the second term of a syllogism, then in place of compulsion, it not only allows but requires an audience to link the first and third terms through its inescapably polysemic stock of common sense. Rhetorical logic is rationally constrained yet open to the arational sense pervading the sociologies of knowledge. Since rhetoric implies the essentially contestable,³⁸ or since it joins utterance to action through arguable interpretation (cf. Bygrave 1993), it is a version of Mannheim’s reconciliation of rational, political and hermeneutic moments.

Mannheim’s (1927a) account of the co-emergence of liberalism, marxism and conservatism likewise implies the same dynamics as in the division of rhetoric into deliberative, forensic, and epideictic genres. The deliberative, like liberalism, entails the urging of reasonable/rational change. The forensic, like marxist ideological

³⁸ While Gallie’s (1964) ‘essentially contestable’ was quite differently grounded, it is consonant with rhetoric. (Recalling Kuhn’s, his argument entails a stress on ‘exemplars’).

analysis, is an 'unmasking' process. The epideictic, or the celebration of shared values, is conservative, being "practised by those who ... defend the traditional and accepted values ... not the new and revolutionary values which stir up controversy and polemics" (Perelman and Olbrechts-Tyteca 1958: 51). Since the deliberative was directed to the future, the forensic to the past, and the epideictic to the present, no one of the three was possible in isolation, just as Mannheim's liberalism, socialism and conservatism were each preconditional for the others. A similar homology occurs between the symbolic generalisations, models and exemplars of Kuhn's disciplinary matrix and Cicero's 'offices' of the classical orator; he should "instruct his listeners, win their sympathy, and vigorously move their emotions" (*The Brutus*: 278).³⁹ In either case, the trinary yields an interactive openness not available under binary categories.

The rhetorical effects of binaries and trinaries are among the aspects of the inductive codifications of classical rhetoric which have been empirically confirmed, for conversation-analysts have shown that in both political speeches and everyday conversations the rhetorical devices most successful in eliciting agreement or applause are antitheses and triply structured lists (Atkinson 1984; Heritage and Greatbatch 1986; Jefferson 1990; Potter 1995: 195-7). Against the same context of shared understandings, the first is always a partisan foreclosure - which side are you on ? - while the second implies the generality of the instances listed; to order by threes is to suggest that more cases could always be mentioned. Simmel (1908a) made a similar point in his discussion of the interactional possibilities of small groups. His noting that "the triad is a structure completely different from the dyad, but not, on the other hand, specifically distinguished from groups of four or more members", and that the "appearance of the third party indicates transition, conciliation, and abandonment of absolute contrast" (Simmel 1908: 141, 145), implied just the difference between fixity and fluidity found between epistemology, on the one hand, and the rhetorical tradition and the sociologies of knowledge on the other.

The tension between dyadic and triadic orderings has been used throughout this chapter; three phases in the sub-/metadiscipline were treated, and each by contrast.

³⁹ As Bacon's (1605: 177) put it: "the duty and office of rhetoric is to apply reason to imagination for the better moving of the will".

That move is now made explicit, in the terms for a sociology of disciplinary knowledge.

TOWARDS A SOCIOLOGY OF DISCIPLINARY KNOWLEDGE: REPRISE

This chapter has comprised a reading of the sociologies of knowledge as both topic and resource. They are required in the first sense for the formal pole of the thesis - that sociologies of knowledge are representations of discipline - and in the second for the informal: that representatives of the discipline act as sociologists of knowledge. Those meta- and subdisciplinary moments will now be summarised, through a return to actor-network theory. It will be shown to be distinct in its accommodation of recurrent difficulties of the field but at the same time to be homologous with much work in it. Then, and on that basis, aspects of the sociologies treated throughout the chapter will be enrolled for subdisciplinary study.

Actor-network theory and the metadisciplinary

Since actor-network theory was used to define the puzzles of this chapter, it is hardly surprising that it accommodates them. But since it was derived in part from the sociologies of knowledge, it is open to the same reading. The 'translation chains' which Callon, Latour and Law use to suggest that 'nothing special' happens in laboratories, and to extend their accounts of the co-production of technology, scientific knowledge and the social to more general social theory, entail both the 'strain towards dualism' repeated throughout the meta-/subdiscipline and the trinary alternative to it.

Actor-network theory was read as an account of 'representation' in which the distinction between political and epistemological senses of the term was collapsed. Epitomised in 'Mannheim's paradox', the effects of the distinction, were seen to pervade the sociologies of knowledge. Acceptance of epistemology as foundational, as in Merton's work, implied preemption of the circularity of any study of knowledge. Barnes' and Bloor's defence of relativism implied the same effect, in the antithetical structuring of the arguments. The 'translation' of actor-network theory, as a contingent mechanism for

the achievement of representation, and as a process in which the epistemological and the political are always already interfused, allows an approach to that problem.⁴⁰ The crucial move is an insistence on symmetry. This is a refusal of dualism and an acceptance of the difficulties consequent on the potentially dualistic character of the refusal itself. That is seen in the actor-network theorists' adaptation of the strong programme.

This was read as an epitome of the sociologies of knowledge, and the actor-network theorists have made similar use of it, as in the fit between Law's (1994) symmetrical, non-reductionist, recursive and reflexive 'relational materialism' and the impartiality, symmetry and reflexivity of the strong programme. Causality, however, the fourth principle of the strong programme, posed difficulties. Attribution of causes required that knowledge of nature, scientific knowledge, be 'explained' by social factors, as if these were unproblematic. But if 'nature' and 'the social' alike are seen as emergent effects of networks, and if "society is no more obvious or less controversial than nature, [then] sociological explanation can find no solid foundation" (Callon 1986: 199). Once the causal principle is adopted, the otherwise open impartiality and symmetry collapse into dualisms. So the maintenance of symmetry requires that causality be foregone, and the 'solid foundation' of actor-network theory then lies in avoidance of foundational claims. 'Mannheim's paradox' is constitutive of it, and through that inclusion it allows a lifting of the purifications in the sociologies of knowledge.

But that actor-network theory also entails a 'strain towards dualism' is evident in depictions of it as a departure from those sociologies. Law (1986a), for example, read them as having passed through three phases. Initiated in Marx's and Durkheim's analyses of the relation between social structure and knowledge, the first culminated in Mannheim's and Merton's epistemological problems. Dating from the 1960s, the second included disparate movements: Levi-Strauss' structuralism; Berger and Luckmann's dialectical phenomenology; Althusserian ideological analysis; enrolments of Kuhn; the recovery of Durkheim by Bernstein and Douglas; and such sociologies of scientific knowledge as the strong programme. In a Kuhnian move, Law held that this second

⁴⁰ From Callon and Latour's (1981) reading of Hobbes, through Latour's (1988b) of Machiavelli, to Latour's (1991) and Law's (1994) analyses of modernity, actor-network theory has always been a political theory.

phase was in crisis, and that a third was emerging, characterised by a Foucauldian questioning of the link between knowledge and structure, of the place of discourse, and of the 'microphysics of power' evident in the relation of knowledge and social control. In this, actor-network theory was among the "work that has gone some way to eroding the basis of the sociology of knowledge as this has traditionally been conceived" (Law 1986a: 2). That rhetoric of oppositional newness recurs in Callon's (1995: 50) call for "a deep reformulation of social theory".⁴¹ Latour's (1991: 54) dualistic use of dualism as the bad guy in his manifesto for a 'new constitution' shows the tension of the move:

Dualism may be a poor solution, but it provides 99 per cent of the social sciences' critical repertoire, and nothing would have disturbed its blissful asymmetry if science studies had not upset the applecart.

But the actor-network theorists have increasingly rejected that 'poor solution'. It is a far cry from an early claim that the "distinction between 'normative' and 'interpretive' modes in sociology is fundamental" (Law and French 1974: 581) to the orderings of relational materialism. The same shift is evident in Latour and Woolgar's (1979/1986) dropping, in their second edition, of the 'social' from their original subtitle of 'the social construction of scientific facts'.

This maintenance of symmetry is occasionally presented in the same terms as Mannheim's solution to antinomies: replacement of binaries with trinaries. Thus in his case-study of the creation and maintenance of Portuguese imperial networks, a case generalised to all translation chains, Law (1986b) proposed that their 'heterogeneous engineering' comprised a triadic assemblage of 'documents, devices and drilled bodies'. Similarly, he claimed in his account of modernity to be working within:

the three-cornered theoretical space between the pragmatism, contingency and process of symbolic interaction, the similarly contingent, technologically sensitive Machiavellianism of actor-network theory, and the attractive necessities of discourse analysis (Law 1994: 86).

More strikingly, Latour (1991: 6, emphasis removed) resolved a 'critical tripartition' in the study of science and the social by describing translation chains as "simultaneously real, like nature, narrated, like discourse, and collective, like society". That structure should not be overstressed - it is not a Mannheimian architectonic principle in actor-

⁴¹ Shapin (1996: 65) found such rhetoric in the 'scientific revolution': "[n]othing so marked out the 'new science' of the seventeenth century as its proponents' reiterated claims that it *was new*".

network theory - but it does suggest that maintenance of symmetry is less the Kuhnian shift which Law claimed than a reprise of the 'continually receding viewpoint' of Mannheim's sociology of knowledge.

On that basis, actor-network theory can be read as continuous with the trinary avoidance of foreclosures found intermittently in the sociologies of knowledge. The homology is shown here, in Table 2.2, through Mannheim's (1925) methodology, Kuhn's (1970b) 'disciplinary matrix', and Latour's (1991) account of networks.

Table 2.2: Trinary structurings of knowledge and its study

Mannheim Sociology of knowledge	Kuhn Disciplinary matrix	Latour Networks
Science	Symbolic generalisations	Real like nature
Historicism	Analogical models	Narrated like discourse
Phenomenology	Lived exemplars	Collective like society

The evident fit between the three orderings, in detail and in structure, will be used to set the formal pole of the antimetabole of the thesis: that the sociologies of knowledge are representations of discipline.

To translate that structure into an architectonic principle is then to adopt a version of Mannheim's (1922: 58) claim that every epistemological systematisation is triadic, a metadisciplinary ambition which echoes claims that triadic schemata are fundamental.⁴² But the reading in this chapter dictates more limited claims. As the next simplest structure after dualism, ordering by threes is qualitatively different, as Simmel held, and has the instability deriving from its rhetorical effects of both climactic closure and open generality (Potter 1995: 196). Always on the assumptions of

⁴² e.g. Peirce's (1931-5) Firstness, Secondness and Thirdness (cf. Stearns 1952; Sheriff 1989); Dumezil's (e.g. 1968) Durkheimian proposal that ordering by threes is a deep structure in the Indo-European cultures; or the trinitarianism of Kroker's (1986) reading of Cochrane's (1940) reading of Augustine. Eco's (1980: 444) Abbot Abo gave an account of popular versions: "three is the number of the Trinity, three were the angels who visited Abraham, the days Jonah spent in the belly of the great fish, and the days Jesus and Lazarus passed in the sepulcher; three times Christ asked the Father to let the bitter chalice pass from him, and three times he hid himself to pray with the apostles. Three times Peter denied him, and three times Christ appeared to his disciples after the Resurrection. The theological virtues are three, and three are the holy languages, the parts of the soul, the classes of intellectual creatures, angels men and devils; there are three kinds of sound - vox, flatus, pulsus - and three epochs of human history, before, during and after the law".

that uncertainty and of a continuity from rhetoric to the sociologies of knowledge, the metadisciplinary character of these last will be read as a recursive enactment of networks which are 'simultaneously real, like nature, narrated, like discourse, and collective, like society'.

Given that account of the sociologies of knowledge, as metadisciplinary topic, elements of them may be enrolled, as subdisciplinary resources.

Subdisciplinary enrolments

The limits of the account must be emphasised. Generality has been claimed for a rhetorical structuring in the sociologies of knowledge, but with scant attention to the particulars of their own existential determination. Continuity has been stressed over the specificities of discontinuity. The 'documents' of a translation chain have been separated from 'devices' and 'disciplined bodies'. But if rhetoric is construed as action the account has remained sociological and so is open to the circularly sociological deployment which was the aim of this chapter.

A trinitarian ordering of the field allows the writers discussed throughout this chapter to be enrolled, but without their purifying responses to 'Mannheim's paradox'. So long as the actor-network sense of symmetry is retained, and with it the 'transition, conciliation, and abandonment of absolute contrasts' which Simmel attributed to triads, then elements of the sociologies of knowledge, of science, and of scientific knowledge remain available for use in subdisciplinary studies.

- The work of purification in Scheler's dual ontologies requires bracketing, but the accommodation of the arational and the sense of emergent rationality given in the interaction of his 'real' and 'ideal' factors both remain as crucial. So are his axiom that all knowledge is an effect of the 'with-world' and the transhistorical and processual permanence in his Pascalian defence of it.
- Mannheim is an obligatory point of passage. His work exemplifies both the tensions of the study of knowledge and the legislative results when tension is lost.

- Merton must be included in any account of sociology, let alone of the sociologies of knowledge. Once the purification in his distinction between culture and civilisation is lifted, subdisciplinary work must accommodate his institutional emphasis, his stress on the normativity of the achievement of knowledge, his call for rigour in the study of 'real factors', and his exemplification of the effect of sociological writing.
- Kuhn is another permanent presence. His general politico-linguistic model of science and the more specific fit between his 'disciplinary matrix' and Mannheim's sociology of knowledge are self-exemplifyingly exemplary.
- Bloor's and Barnes' 'strong programme' was read as a performative summary of the study of existential determination. Once the latent purification in their principle of causality is suspended in favour of their principle of symmetry, their representation of a tradition of representations is a model.
- The 'interpretative repertoires' in Mulkay's discourse analysis are one means of representing an agonistic tradition. More specifically, his identification of the same repertoires in scientists' daily practice, in celebrations of scientific achievement, and in parliamentary debates over science is a concrete exemplar of trans-scalar generality.

A translation of those elements can be applied to any study of the legislative. It will be directed throughout the thesis to a sociology of disciplinary knowledge.

Towards a sociology of disciplinary knowledge: Reprise

The isomorphism between Mannheim's methodology, Kuhn's disciplinary matrix, and actor-network theoretical translation chains, and the enrolments enabled by a trinitarian reading of it, meet the objective of a non-foundational foundation for a study of disciplinary knowledge. Mannheim's 'knowledge', Kuhn's 'discipline', and the actor-network theorists' 'representation' set the basis of the thesis. An academic discipline is

a disciplinary matrix or problematic translation chain, stabilised through formal organisation, and, on the assumption of trinitarian generality, the knowledge generated within it is then an effect of documents, devices, and drilled bodies.

After the distinction between formal and folk sociologies has been illustrated in a case-study of Wirth's {1947} and Gamson's {1994} addresses, the simultaneously sub- and metadisciplinary character of the triadic homology will be elaborated in the next two chapters, in a modelling of the disciplinary and in a survey of methods used in the study of disciplinary knowledge.

CASE 2 : WIRTH'S AND GAMSON'S 'WE'

Disciplinary knowledge is knowledge in a with-world. That axiom from the reading of the formal accounts will be the basis for treatment of presidential addresses as a folk version of them. The practical separation of formal and folk will be prefigured here, through apparently odd omissions in Wirth's {1947} and Gamson's {1994} addresses. Both included discussion of a global 'we', both were situated within a disciplinary 'we', and both resemble the sociologies of knowledge, but the linking of subdiscipline and occasion is missing from both.

Of the sixty presidents under study, Wirth might most have been expected to couch his address as sociology of knowledge: he was Mannheim's co-translator and his sponsor in American sociology;¹ he taught the subdiscipline at Chicago;² he stressed the consonance of Mannheim's early work with the American pragmatism subsumed in Chicago sociology; he shared his later views on social engineering; he was president in the year of Mannheim's death. That he did not even mention him is one symptomatic silence; Wirth's address was Mannheimian, but tacitly so. Then, of the sixty presidents under study, Gamson used a method closest to that being developed here, for his version of Goffman's frame-analysis was also a version of the 'repertoires of contention' widely used in political sociology,³ which in turn recall Mulkay's independently derived use of the term. His address, too, was symptomatically silent. Without reference to Wirth, Gamson echoed him in taking the nature of 'collectivity' as his topic, he used the same locational trope, he had the same substantive focus, and he drew similar conclusions; Gamson's address was Wirthian, but tacitly so. Those elisions in the two addresses will be used to illustrate the preemption of subdisciplinary attention to the legislatively metadisciplinary in presidential addresses.

¹ Albeit a rather ambivalent one, as Kettler and Meja (1995: 193-246) have shown.

² Wirth claimed the sociology of knowledge as a major interest, while noting that it was "a field which is misnamed and with the misnaming of which, unfortunately, I have had something to do. It should rather be called the sociology of intellectual life. ..." (in Odum 1951: 231).

³ e.g. Tilly (1979); Simons and Mechling (1981); Gamson (1988); Snow and Benford (1988); Traugott (1995). Since some of these include enrolments of Goffman's (1974) 'schemata of orientation', and since that repeats Mannheim's (e.g. 1928a: 238) use of the phrase, 'repertoires' are a typical sociological palimpsest.

It was shown in the note on Goffman {1982} that the occasioning of presidential addresses entail both personal defence of a vision of sociology and renewal of the collectivity of sociologists. Wirth made that conjunction explicit in opening an address which he depicted later as a summary of "[w]hat I think about sociology and its task in the present day world" (in Odum 1951: 232), and which Bendix (1954: 529) was to describe as "obviously intended as a programmatic statement":

On such an occasion as this it is customary to offer a discourse on one's favorite topic, under unusual conditions of freedom bordering on license. If one is so inclined, he is even permitted to preach a sermon, though it is well to remember that the congregation is free to depart before the benediction.

Beyond the disciplinary self-situating of his own 'favorite topic' of sociology, Wirth located himself in the immediacy of Hiroshima and Nagasaki. Claiming that '[t]he physical knowledge that threatens to destroy us is obviously not matched by social knowledge requisite to save civilization', he called for sociologists to contribute to the 'social psychological integration of the human race', for '[u]nless we solve that problem, and solve it in a reasonably satisfactory way soon, there will be no opportunity to work on any of the others on which our minds or our hearts are set'. Study of the mass media was crucial to this:

The atomic bomb will not ... yield to a physical defence or a counter-weapon which will neutralize its destructive potential. The only defence we have is social - the creation of world consensus. Since the mass media of communication are capable of providing the picture of social reality and the symbolic framework of thought and fantasy and the incentives for human action on an enormous scale, the knowledge of their effective use should become the most important quest of social science, and particularly of sociology.

Since Wirth saw such a 'quest' as entailing knowledge of collectivities, and since the 'central task' of sociology was 'to understand the behavior of men in so far as that behavior is influenced by group life', his metadisciplinary intent was clear. As a starting-point, he set out a disciplinary common sense of the mechanism by which 'we groups' were sustained; sociologists had traditionally attended to:

the consensus that derives from the social heritage of a people, from a common culture, a common history and set of traditions, from the mores, which can make anything seem right, true, good, beautiful and possible. It is this basis of common social life as patterned by these traditions that makes it possible in the last analysis for any group to think of itself and to act as a society, to regard itself as a 'we' group and to counterpose this 'we' experience to all that is alien. The extent to which force and authority, law, religious sanction and leadership, propaganda and education, and the apparatus of symbols can be used effectively depends in large part upon this substratum of a common basis of

knowledge, belief and standards molded by tradition and reinforced by the ongoing social life which embodies that tradition.

Given the occasioning of the address, that account of a tradition embodying an ongoing social life might well have been applied to sociology itself. But Wirth's imaginary of order recalled Mannheim's legislative sociology of planning more than the sociology of knowledge. The address was then consistent with his introduction to *Ideology and Utopia*, where he held that the sociology of knowledge "historically and logically falls within the scope of general sociology conceived as the basic social science" (Wirth 1936: xxvii). His 'Reform Darwinism was identical with Mannheim's (1943: 119) vision of a place "in every planned society [for] a body somehow similar to the priests, whose task it will be to watch that certain basic standards are established and maintained".⁴ Or, like Mannheim, he depicted sociologists as exemplifying the order they were called to direct:

In the world of science we come about as near to a world society as in any phase of human life, and this world-wide scope of communication which science exemplifies might well serve as a model to be approximated in other realms of human experience, for science, including perhaps even social science and philosophic scholarship, has proved its power to surmount local, national, sectarian and class barriers, and even to infiltrate through the obstacles of official censorship.⁵

But Wirth neither made all those elements of a Mannheimian sociology explicit, nor used them to link the occasion and the subdiscipline. That silence suggests a theme which Reiss (1964: xii) recalled from his lectures, his dismissal of theories of social action on the grounds that "that they focused only on people doing things..., when failure to act was in his judgment an equally significant fact about human social life". In this case, the 'significant fact' of Wirth's silence, a silence repeated in most of the addresses, will be construed throughout the thesis as less a failure than an occasioned disruption of an

⁴ Kuklick (1980: 825) used 'Reform Darwinism' to describe the 'Chicago sociology' which Wirth exemplified: "Unlike the advocates of laissez faire labelled 'Social Darwinists' who deplored social activism as interfering with the natural process of evolution, Reform Darwinists saw calculated social reform as itself evidencing an evolutionary advance of an unprecedented order, demonstrating human attainment of new levels of rationality and self-control". Wirth's students have stressed its identity with Mannheim's work (e.g. Reiss 1964: xxviii; Bendix 1954: 529). Janowitz (1970: 117) noted the 'entrepreneurial overtone' in this.

⁵ This recalls Merton as much as it does Mannheim. A very similar, but 'left-wing Kuhnian', account is found in Rorty's (e.g. 1987) pragmatic depiction of science as a model of solidarity. cf. Delaney and Widdison (1990). Sorel (1921) gave an early pragmatic reading of science.

occasioned reflexivity.

Now Wirth held office during a transitional phase in American sociology; his presidency was two years before Parsons' {1949}, which might be taken as inaugurating the golden age of structural-functionalism, and his vision of sociology was temporarily eclipsed. Where he held that "to be a better social scientist, the scholar must be a more active citizen ... [as] a matter of professional competence, not of individual preference" (Bendix 1954: 529), Weberian *Wertfreiheit* became the 'ruling myth in sociology' (cf. Gouldner 1961).⁶ But under changed conditions, his work again became potentially salient, and even, as Goffman {1982} suggested, an orthodoxy:

We all agree, I think, that our job is to study society. If you ask why and to what end, I would answer: because it is there. Louis Wirth, whose courses I took, would have found that answer a disgrace. He had a different one, and since his time his answer has become the standard one.

The assumptions beneath Wirth's 'answer' pervade Gamson's {1994} address on 'the politics of exclusion'. Given further links between the two addresses, it then appears odd that just as Wirth did not mention Mannheim, Gamson did not mention Wirth.

As Mannheim's translator, Wirth was a presence in the previous chapter, and the same could be said of Gamson. In a reconciliation of epistemological and political senses of representation, the 'repertoires' of the sociologies of knowledge have been independently used in Gamson's specialty of the study of social movements. The 'ordering' being developed in the thesis has much in common with the 'political culture' suffusing the 'frames' which are his version of them.⁷ He defined this as:

the meaning systems that are culturally available for talking, writing and thinking about political objects: the myths and metaphors, the language and idea elements, the frames, ideologies, values and condensing symbols (Gamson 1988: 220).

His address rested on that definition. But since he did not treat the 'political culture' of sociology itself in his discussion of 'we groups', he showed the same blunted reflexivity as had Wirth.

⁶ Gouldner's 'Anti-Minotaur' was itself a presidential address, to the SSSP.

⁷ In one version, Gamson distinguished "framing devices that suggest how to think about [an] issue and reasoning devices that justify what should be done about it. The five framing devices are (1) metaphors, (2) exemplars ... (3) catchphrases, (4) depictions, and (5) visual images ... The three reasoning devices are (1) roots (i.e. a causal analysis), (2) consequences (i.e. a particular type of effect, and (3) appeals to principle (i.e. a set of moral claims) ... (Gamson and Modigliani 1989: 3-4).

While Gamson did not situate himself in the occasion, he did share Wirth's more general location. In his work on social movements he had treated responses to nuclear power (e.g. Gamson 1988; Gamson and Modigliani 1989), and that was the source of his opening trope:

On this day, the 49th anniversary of the nuclear dawn, our world is full of contradictions. On the one hand, there is abundant evidence of an increasing global consciousness. ... On the other hand, we live in a world of increasing fragmentation...

In 'open historical moments such as the present, when culturally dominant frames have broken down and new ones have not yet become established', the 'anxiety with an edge of terror' brought by 'the new nuclear age' was being repeated. Just as Wirth had called for attention to 'world consensus', Gamson looked to a disciplinary contribution to 'global consciousness', and like Wirth he set out a mechanism by which collectivity was established and maintained. Under the rubric of the 'politics of exclusion', he treated the 'cultural contest ... over who is the 'we' to whom specific moral obligations apply, and who is the 'they' to whom they do not':

Many different universes of obligation exist simultaneously, each with its own set of rules about how members are to be treated in contrast to outsiders. Only some of the obligations that we feel we owe to our family members are extended to our friends and acquaintances; only some of these extend to other members of the various collectivities with which we may identify - neighborhoods, organizations, movements, and solidarity groups of various sorts; only some of the moral obligations we extend to members of our own society are extended to members of other societies.

While holding that resolution of the uncertainty of the times required an acceptance of difference, Gamson nonetheless stressed the intractable contradictions of the 'multi-identity politics' found in social movements, and which were characteristic of the 'new world order' generally:

How does one reconcile [minimisation of the effects of difference] with the fact that a given campaign often challenges exclusion on the basis of a particular identity? And how does one acknowledge that the 'we' is diverse and has fuzzy boundaries while operating a political opportunity structure that supports inclusion claims only when they are translated into a primary identity group, or, better yet, as a distinct market? And how does one validate these multiple identities in practice without impaling oneself on the horn of divisive internal conflict and fragmentation?

Wirth had held that '[m]ass communication is rapidly becoming, if it is not already, the main framework of the web of social life', and that was a theme of Gamson's substantive work as well: media discourse and public opinion are parallel systems for the construction

of meaning, for example (Gamson and Modigliani 1989), or social movements and mass media are interacting systems (Gamson and Wolfsfeld 1993). He repeated that in his address, in claiming that '[c]ultural visibility in America means media visibility. The absence of a media voice is a primary measure of cultural exclusion'. Finally, even if he could 'offer no clear action formulas', Gamson's stress on the 'dilemmas and potential problems' in multiple identities and in attempts to use the media was intended in the same ameliorative sense as Wirth's finding it 'tempting to exhort one's fellows in the scientific world to live up to the great responsibilities which the sorry state of the world might be presumed to devolve upon them'. More modest in his metadisciplinary assumptions, he nonetheless echoed Wirth's programmatic call for sociological knowledge to be used in the engineering of consensus:

'Never again' may be an undeliverable promise, but it still remains a noble vision. If sociology can contribute to a clearer understanding of the politics of exclusion, relevant social actors will be better able to devise strategies for preventing and reversing processes of exclusion.

Mutatis mutandis, then, Wirth's focus on 'consensus and mass communication' and Gamson's on the place of the media in 'the politics of exclusion' are strikingly similar. The addresses are also alike in illustrating the interpenetration of the formal and folk poles of this thesis, Wirth's through the sociology of knowledge, Gamson's through his 'frames'. They are similar, finally, in the absence of reflexivity.

It was suggested in the note on Goffman that the presidents are situationally required to reaffirm the collectivity of the discipline through their own version of it. Wirth certainly did that in his programmatic call, but given his connection with Mannheim it is at first glance odd that he neither used the sociology of knowledge to do so nor reconciled the scientific collectivity of sociologists with their 'social heritage ... common culture ... common history and set of traditions'. Gamson's missing reflexivity was just as peculiar. Despite occasioned requirements, he did not extend the identities and exclusions generated within 'our own society' to the Association he was addressing, nor the 'cultural contest' and 'fragmentation' on a global scale to the widely diagnosed equivalents within the discipline. His repetition of Wirth's tacit distinction between

formal and folk, and thus the reassertion of an epistemological against a political 'we', suggests that what is situationally required may also be situationally debarred; that will be one theme developed throughout the thesis. Then, given the similarities in problematic and treatment between his address and Wirth's, the absence of reference to them suggests another theme of the thesis: an echoing in the sociologies of knowledge of the disjunctive continuities in the discipline at large.

For the moment, it is enough that the folk 'we-ness' of presidential addresses has been contrasted with the formal 'we-ness' of sociological study. Those two senses of collectivity are elaborated in the next chapter, in a modelling of the disciplinary.

CHAPTER 3 : MODELLING THE DISCIPLINARY

Sociological knowledge is modern knowledge, modern knowledge is disciplined knowledge, and the sociologies of knowledge are then representations of discipline. One phase of that argument was developed in the previous chapter, where, on Latour's understanding of the modern as simultaneously 'work of translation' and 'work of purification', the sociologies of knowledge were seen to have entailed the expected separation of the political and epistemological senses of representation. Continuing the argument, the disciplinary will be modelled in this chapter to accommodate the circularities of 'Mannheim's paradox'.

'Discipline' epitomises the discipline of sociology's location in what is studied within it, for although the concerns and methods of sociology may always have been argued, the arguments have always involved the formal processes subsumed in Weber's (1914: 253) ideal-type of 'discipline'. It was:

nothing but the consistently rationalized, methodically trained and exact execution of the received order, in which all personal criticism is unconditionally suspended and the actor is unswervingly and exclusively set for carrying out the command.

That sense was focal in his own work and evocative as well of such uses as Comte's (e.g. 1830-42 II: 51) "discipline in [the] spirit of positive rationality" or Marx's (e.g. 1867: 480-91) linking of 'discipline' and the division of labour in 'the capitalist character of manufacture'. Given the genealogy of the sociologies of knowledge, it is not surprising that the fusion of 'discipline', 'the modern', 'the received order', and 'the rational' are found in them, but it does mean that they can be taken as paradigmatic of both more general processes and the study of them.¹

The simultaneously meta- and subdisciplinary modelling required for that circularity is an extension of what has been suggested by the actor-network theorists. As an aside, for example, Law noted the "ramshackle and patchy" character of a discipline (Law and French 1974: 591), Bowker and Latour (1987: 729) observed that Foucault had opened "the way to treating formal disciplines ... as ways of disciplining, as agents of

¹ van Krieken (1990: 353-4) gives a compact summary of the continuing salience of 'discipline' in sociology.

social order", and Law (1986b) drew on the same sense in his 'documents, devices and drilled bodies'. But while they have consistently linked scientific knowledge and its study to social order, they have rarely used 'discipline' to do so.

To represent formal accounts of knowledge and to derive a means for comparison with the informal, the model will be developed through an ordering of exemplars, by Latour's version of the triad from the previous chapter. If the network of a discipline is 'simultaneously real like nature, narrated like discourse and collective like society', then 'discipline' is an effect of disciplinary science, disciplinary discourse, and disciplinary culture. Each has been stressed in accounts of disciplinary knowledge. Merton's use of 'discipline', in a variation of his distinction of civilisation from culture, exemplifies the first moment; the second is found in Foucault's neo-Weberian (and neo-Baconian) account of the disciplinary society; and Geertz's call for an anthropology of disciplines enacts the third. Through the recurrence in them of work of purification, each of these will be found to require supplementing with the other two.

To accommodate that co-performativity and the more general imbrications of disciplined knowledge, the trinitarian reading will be reduced to a trinitarian model. The disciplinary will be represented as an effect of 'disciplinarity', 'disciplining', and 'discipline', where the first is an optimistic imaginary of 'science', 'collectivity', and 'usefulness', the second a diffuse set of normalising practices, and the third a failing ensemble of 'documents, devices and drilled bodies'. That modelling is applicable to a metadisciplinary reading of the sociologies of knowledge and to subdisciplinary study.

DISCIPLINARY SCIENCE

From Plato and Aristotle through Descartes to Vico, Kant and Comte, any number of writers in the western tradition have codified the acquisition and standing of knowledge, and increasingly since the seventeenth century 'discipline' has been used in such accounts to describe procedurally warranted certainty. As Shumway and Messer-Davidow (1991: 210) have stressed, however, the equation of learning and rule has a long history. Already in classical Latin *disciplina* denoted not only the military power noted by

Weber (1914: 261), but systematic knowledge as well. Those two senses were generalised under monasticism, where 'discipline' was a synonym for 'rule', and by the mediaeval differentiation of universities from monastic and cathedral schools the word was used in its contemporary sense of a branch of knowledge. Pertaining to the 'disciple' or scholar, 'discipline' was contrasted with the 'doctrine, as the property of the teacher or doctor. 'Discipline' became associated with practice or exercise, while 'doctrine' denoted abstract theory. When scholasticism was rejected in favour, *inter alia*, of Baconian inductive inquiry, 'discipline' was then available as a descriptor:

To call a field a 'discipline' is to suggest that it is not dependent on mere doctrine and that its authority does not derive from the writings of an individual or school but rather from generally accepted methods and truths (Shumway and Messer-Davidow 1991: 202).

That equation of 'science' and 'discipline' is one orthodoxy, if not itself a 'doctrine'. Merton's use of 'discipline' will be illustrated first, and then used as a benchmark for discussions of the term within and beyond sociology.

Merton on discipline

Merton was shown in the previous chapter to have focused in his sociology of science on rationality and rationalisation (his doctoral thesis was in part an extension of Weber's account of Puritanism), and in the process to have privileged epistemology. While 'discipline' was not among his key terms, he did use it in this sense; a science required technical norms, or agreed criteria of evidence:

Only when a scientific discipline reaches the point of no agreement, a degree of dissensus where scientists persistently disagree on what will be regarded as acceptable evidence of validity, does the 'discipline' break down (Merton 1971: 196, emphasis removed).

Similarly, when he talked of the development of "a conceptually discrete discipline, able to command its own tools, techniques, methodologies, intellectual orientations, and problematics" as the "creation of a *cognitive identity*" and isolated that "facet of the institutionalization of a field of learning" from a "parallel set of shifts ... revolv[ing] around the creation of a *professional identity* for the new enterprise" (Thackray and Merton 1972: 473), he recalled his earlier distinction of civilisation from culture. This version of his standard move was more layered. Just as the cognitive was an effect of rational rules, so 'professional' in Merton's (e.g. 1958) sense was a matter of 'rigorous

standards'. Thackray and Merton (1972: 494) described 'cognitive' and 'professional' as each in turn being separable from 'culture':

To create a cognitive identity for a discipline requires ... formation of that sense of common orientation and purpose which both springs from and nourishes agreement on central problems and on the relevant techniques of conceptualization and analysis. Similarly the creation of a professional identity is not guaranteed by the formation of learned societies and the production of extensive propaganda, necessary though these are. It also requires the recruitment of followers and students and more especially the creation of satisfactory career structures. These latter requirements depend on structural and cultural shifts within society at large and cannot be directly commanded or engineered.

Since the two identities were "inextricably interwoven in the daily practice of any fully institutionalized field of learning" (Thackray and Merton 1972: 474), their separation was analytical only. So if the epistemological basis of analysis is queried, so too is the distinction.

Now Merton's equation of 'discipline' with 'science', and its further distinction from 'profession', seems to have been a disciplinary common sense. It appears, for example, in debates over the professionalisation of American sociology itself.

Mertonian common sense, within sociology

Parsons (1959) gave an exemplary account of 'discipline' and 'profession' in this context, and since Merton had used his work on professions to develop 'the norms of science', this was also a fusion of the disciplinary and the subdisciplinary. Occasioned as analytical and as organisational (in a controversy between psychologists and sociologists),² and written by a central disciplinary figure, this widely cited paper appears to document a common sense. Unlike other participants in the controversy (e.g. Borgatta 1958; Goode 1960), Parsons did not rue professionalisation as such. Sociology was a profession, so long as the term denoted:

a category of occupational role which is organized about the mastery of and fiducial responsibility for any important segment of a society's cultural tradition, including responsibility for its perpetuation and for its further development. In addition, a profession *may* have responsibility for the application of its knowledge in practical situations (Parsons 1959: 547, emphasis added).

² The paper was commissioned by the executive of the then American Sociological Society in partial response to perceived disciplinary threat - psychologists had sought a form of state registration which would have prevented the teaching of social psychology in departments of sociology - and was first delivered to the meeting at which the ASS became the more 'professional' American Sociological Association.

Where professions such as medicine entailed the provision of service, Parsons (1959: 547) held that sociology was:

universally conceived as a scientific discipline which is clearly *primarily* dedicated to the advancement and transmission of empirical knowledge in its field and *secondarily* to the communication of such knowledge to non-members and its utilization in practical affairs.

Sociology was a profession with a disciplinary core, and 'discipline' meant 'science'.³ Its scientificity, however, "stands at present in an early stage" (Parsons 1959: 548), and its 'field' - the social system - was not well differentiated, either organisationally or substantively, from those of political science, anthropology, economics, or psychology. As in the psychologists' move, this might give rise to "serious complications both for the discipline itself and for those closely related to it in the scientific community" (Parsons 1959: 556). Sociology then had a duty of 'citizenship', with other disciplines and cultural groupings. Since 'academic citizenship' implied responsibilities beyond the discipline proper, scientific functions were basic but the professional sociological role could not be limited to them. While sociologists were increasingly enhancing the cultural 'definition of the situation', in teaching, in the media, or in more general treatment as experts, they risked "ideological contamination" (Parsons 1959: 552). The more they were externally engaged the more it was necessary "that [the discipline] serve as a primary guardian of the scientific tradition, counteracting the many tendencies to introduce bias and distortions" (Parsons 1959: 555).

The form if not the detail of this work of purification - disciplined science is on one side of an epistemological line, contamination and distortion on the other - appears to have been widely shared. In a much reprinted introductory textbook, for example, Inkeles (1964: 106) repeated the distinction: "[s]ociology is not only an intellectual discipline; it is also a profession. ... The nature and practice of a discipline determine the kind of intellectual enterprise and profession it may become". Or, where Inkeles kept Parsons' sense of 'discipline' as a core, Horowitz (1964: 275) held that sociologists could be identified, by 'ethical modalities and epistemological issues', as 'believers in a

³ This denotation supplements Parsons' (1937) earlier usage, where 'discipline' in the sense of constraint or internalised social control was among the elements he claimed as common to Pareto's, Durkheim's and Weber's work.

discipline and believers in a profession':

One finds the professional concerned with developing a picture of the world, a model of the universe, and hence disposed towards an epistemological theory of coherence. One finds the disciplinarian concerned with social interaction, with affective attitudes toward the object of his study, and hence disposed towards a pragmatic philosophy.

Relating "the professional to a specialist's approach and the disciplinarian to a broad intellectual approach" - Merton and Parsons typified the former, Goffman the latter - Horowitz (1964: 277) equated 'profession' with the epistemologically distinct.⁴ But shifts of emphasis or reversals of meanings are of less interest here than the form of analysis. However they are couched, 'discipline' and 'profession' stand as analytical poles, with one or them epistemologically warranted. That move from the sociologies of knowledge is also found in more general discussions of 'discipline'.

Mertonian common sense, beyond sociology

As Parsons implied, any discipline emerges agonistically from relations with others, with the boundaries between them always uncertain,⁵ and the fact that much research is practised at those boundaries - being variously described as inter- or trans- or cross-disciplinary - has increasingly drawn organisational and analytical attention. In such studies, 'discipline' is routinely identified with 'science', but with the addition of a new distinction, between 'disciplinarity' and 'discipline'. In his paper at a widely cited colloquium on interdisciplinary research, for example, Heckhausen (1972: 83) took disciplinarity as the "specialised scientific exploration of a given homogeneous subject matter producing new knowledge and making obsolete old knowledge". Or, in a review of the oxymoronic specialty of 'interdisciplinarity', Klein (1990: 104, emphasis added) held that 'discipline' "signifies the tools, methods, procedures, exempla, concepts and theories that account *coherently* for a set of objects or subjects". While that sense is familiar from the sociologies of knowledge,⁶ it also raises the difficulties found in them:

⁴ In this odd reading of Parsons, Horowitz also misread Weber. He identified the 'disciplinarian' emphasis' with 'sociology as charismatic authority' (Horowitz 1964:280), whereas Weber (1914: 253) held that 'The force of discipline ... eradicates charismatic authority'.

⁵ Abbott (1988) elaborated that sense in his treatment of 'the system of professions'.

⁶ The sociologies of knowledge barely appear in these treatments of the interdisciplinary. Heckhausen and his co-contributors were scientists and philosophers. Klein glossed Merton's 'middle range theory' and Kuhn's *Structure* as interdisciplinary, but did not elaborate.

the issues of how the scientific differs from its embedment; how sciences are to be distinguished; how each is constituted. Echoing Merton's version of what is internal and external to science, Heckhausen (1972: 86) listed epistemological criteria for disciplinary recognition, but added 'historical contingencies'. These might:

speed up or slow down a discipline's development or progress [but] are not exclusively due to the inner logic of the respective subject matter explored by able scientists. Disciplines are also under the sway of extradisciplinary and changing forces, such as public reputation, sociocultural values, political ideologies and economic conditions. The extradisciplinary forces not only control material resources, they determine the climate for growth. Last but not least, external contingencies add up to the *Zeitgeist* of the scientists themselves, influencing their research interests and theoretical preconceptions.

While treatment of such factors as residual enacts Merton's separation of 'civilisation' from 'culture', Heckhausen's and Klein's distinction of 'disciplinarity' from 'discipline' does suggest a Mannheimian third term. 'Disciplinarity' as a state of scientificity and 'discipline' as a scientific practice clearly require a bridging process.

Disciplinary science: Reprise

In surveying treatments of 'discipline', King and Brownell (1966) had reached a similar conclusion.⁷ On the assumption that the plurality of autonomous disciplines was "grounded in the ability of each to define the nature of its own knowledge and its method of getting knowledge", they held that the internal workings of each discipline and the relations between it and others belied any epistemological distinctiveness:

The body of persons that make up a discipline is most often described and named by the *domain* which it stakes out for itself in the larger territory known as the intellectual life. ... the domain of a discipline is that natural phenomenon, process, material, social institution, or other aspect of man's concern on which the members of the discipline focus their attention.

The domain is that which the members of the community claim it to be. No plenary body stipulates boundary lines; no discipline files deeds in academic courthouses; no intellectual tribunal tries claim-jumpers (King and Brownell 1966: 62, 74).

If disciplines were both "the processes and products of man's symbolic efforts to make his experience with the world intelligible", then rather than being procedurally guaranteed a discipline was a "community of discourse" (King and Brownell 1966: 37, 95). The implication in that of scientificity as a continuing achievement opens one means of

⁷ Again, the sociologies of knowledge were not used in this survey; as philosophers of education, King and Brownell were concerned with curricular reform.

accommodating a recurrent tension in the common-sense distinction between 'discipline' and 'profession': a discursive linkage of 'disciplinarity' as an ideal of scientificity and 'discipline' as practised. Thackray and Merton's (1972) 'inextricable interwovenness' of 'cognitive' and 'professional' identities, or Parsons' (1959) 'scientific community' and 'academic citizenship' imply the political structuring elided in their epistemological assumptions. That connection is drawn explicitly in Foucault's work.

DISCIPLINARY DISCOURSE

Like Merton or Parsons, Foucault identified 'discipline' with the 'modern', and like them he linked it to 'science', but his focus on the co-emergence of authority in formal knowledge and in social order made explicit what they left tacit. But his 'discourse' needs to be "cut down to size" (Law 1994: 107), for while it implies contingency, his own usage tended to the deterministic. The correlates to Kuhn's 'paradigm shifts' in his 'epistemic breaks' likewise tend to the same loss of essential tension between tradition and innovation. So Foucault's 'discipline' will be read here as complementary to an equation with 'science', rather than as a replacement of it. In particular, the 'disciplinarity' and 'governmentality' derived from it will allow a simultaneously political and epistemological bridging of 'culture' and 'civilisation'.

Foucault on discipline

Foucault used 'discipline' as a trope of modernity throughout his archaeological and genealogical periods. Treating the study of language, labour and life archaeologically, he distinguished two epistemic strata below the emergence in modernity of the human sciences (Foucault 1966). In disjunctive rationalisations, fields of study became 'the disciplines'. Epistemic unity was to be sought in characteristic 'discursive formations', or 'systems of dispersions' (Foucault 1969: 37); these entailed three aspects of statements - the authority of a speaker; what institutional sites were involved; and how the positions of the subject were defined - and "three types of prohibitions, covering objects, ritual with its surrounding circumstances, [and] the privileged or exclusive right to speak

of a particular subject" (Foucault 1970: 8).⁸ Through regularities in the enactment of these features and prohibitions, 'disciplines' could be described as:

groups of statements that borrow their organization from scientific models, which tend to coherence and demonstrativity, which are accepted, institutionalized, transmitted, and sometimes taught as sciences (Foucault 1969: 178).

Or, since discourse was not restricted to language, disciplines were also:

defined by groups of objects, methods, their corpus of propositions considered to be true, the interplay of rules and definitions, of techniques and tools: all these constitute a sort of anonymous system, freely available to whoever wishes, or whoever is able to make use of them, without there being any question of their meaning or their validity being derived from whoever happened to invent them (Foucault 1970: 15).

Depending on new propositions being generated within 'the realm of the true' defined by it, a discipline then also "repulses a whole teratology of learning" (Foucault 1970: 16).

What was not disciplined could not be within the true, and the true was held by a policing of boundaries. Here, at the cusp of a shift in Foucault's method, 'discipline' acquired an explicitly dual meaning. Disciplines constituted "a system of control in the production of discourse, fixing its limits through the action of an identity taking the form of a permanent reactivation of the rules" (Foucault 1970: 17).

'Rules' were prominent in the 'genealogical' method, by which archaeological knowledge could be tactically fused with contemporary local memories; a 'genealogy' was possible only on the condition that "the tyranny of globalising discourses with their hierarchy and all their privileges of a theoretical *avant-garde* was eliminated" (Foucault 1976b: 83). Taking prisons as epitomising more general processes, Foucault held that the 'microphysics of power' pervading the 'simple instruments' used in them to transform prisoners into 'docile bodies' were also deployed in every emergent institution: the army, the school, the factory, the clinic. As exemplified in Bentham's Panopticon, discipline entailed observability; it was achieved through the invisibility of those in authority and the visibility of the ruled, with each individual constituted "as effect and object of power, as effect and object of knowledge" (Foucault 1975: 187, 192). Treatment of subjects as 'cases' marked the Modern episteme, with individuation "fabricated by this specific technology of power that I have called 'discipline'" (Foucault 1975: 194).

Further, since analysis required analysts, 'discipline' evoked and was an effect of 'the

⁸ Like the sociologists of knowledge, Foucault quite commonly ordered by threes.

disciplines'. If the prison epitomised modernity, and if "[t]he carceral texture of society assures both the real capture of the body and its perpetual observation" (Foucault 1975: 304), it assured as well the human sciences. Any study involving hierarchical observation, normalising judgement and examination of human subjects - psychiatry, psychology, criminology, and, most importantly here, sociology - was implicated in that texture. The disciplines and discipline merged in the modern.

Discipline and disciplinarity

To treat 'discipline' as pervasive has advantages over its equation with 'science'. The simultaneous use of 'discipline' as a general trope and rejection of the 'tyranny of globalising discourses', recalling the tension between the meta- and the subdisciplinary in the sociologies of knowledge, exemplifies and accommodates the 'inextricably interwoven' character of 'discipline' and 'profession' or of 'cognitive' and 'professional identities'. It has been widely adopted in studies of disciplinary knowledges.⁹ On the premise that academic disciplines are "regulated and regulating elements of a larger disciplinary regime" (Shumway and Messer-Davidow 1991: 211), such studies have been subsumed in a field, if not quite a discipline, of 'disciplineness' or 'disciplinarity'. Instead of a 'state of being scientific' (Heckhausen 1972; Klein 1990), 'disciplinarity' refers here to "the *possibility conditions* of disciplines" (Messer-Davidow *et al.* 1993: 1-2), or to:

the coherence of a set of otherwise disparate elements: objects of study, methods of analysis, scholars, students, journals, and grants, to name a few. ... disciplinarity is the means by which ensembles of diverse parts are brought into particular types of knowledge relations with each other (Messer-Davidow *et al.* 1993: 3).

Beyond the Foucauldian stress on the material and the embodied, 'disciplinarity' is then also given as reflexive, being both the achievement of coherence and the study of it. In the latter sense, Messer-Davidow and her colleagues contrasted it with the history of ideas and epistemology, as might be expected, but with the sociology of knowledge as well. While granting that Mannheim had emphasised the effects of the social location

⁹ e.g. Hoskin (1979) and the papers in Ball (1990) on education, Hoskin and Macve (1986, 1993) on accounting, or Hacking (1991) on statistics. Beyond such specialised studies, Foucault has been variously articulated to the study of knowledge; Lenoir (1993) combines 'discipline' with Bourdieu's 'scientific field', for example, or Lynch (1985) gives an ethnomethodological reading.

of knowledge-producers, they claimed that the sociologies of knowledge derived from his work had largely been restricted to 'ideas' and that even when 'new sociologists of knowledge' treated practices pointing to 'discipline' - as Latour and Woolgar (1979/1986) did - they had not drawn the connection:

The failure to do has been attributed to the synchronic, rather than diachronic, approach taken by sociologists of knowledge, but it probably arises from the fact that for them, as for others who concern themselves with academic knowledge, discipline remains a given (Shumway and Messer-Davidow 1991: 217).

They held that while studies of disciplinarity need not be history as such, writers on it differed from sociologists of knowledge in taking knowledge as historically contingent. If that, and their account of Latour and Woolgar as idealists, is an odd description of the sociologies of knowledge, their noting of a subdisciplinary emphasis on 'ideas' is to the point. Shumway and Messer-Davidow (1991: 222) suggested one means for moving beyond them, in their claim that disciplines are "rhetorically constituted",¹⁰ and Foucault's own development of 'discipline' allows a development of that link with the sociologies of knowledge.

Discipline and governmentality

Since Foucault's 'carceral society' has something of the metaphysical pathos of Weber's 'rationalisation', his account of 'discipline' might be seen as just the kind of tyrannously globalising discourse to which he had opposed his work. He had conceded the point. Noting that in his studies of asylums and prisons "I insisted, I think too much on the techniques of domination", he granted that while "[w]hat we can call discipline is something really important in these kinds of institutions ... it is only one aspect of the art of governing people in our society" (Foucault 1980: 204). He turned to the enactment of a simultaneously individuating and totalising 'government', using 'governmentality' to describe study of this rule of 'each and all' (Foucault 1981).

It was to be understood in three senses: as an ensemble of institutions, procedures, analyses and reflections which allowed a targeting of populations; as a tendency which led to the preeminence of a 'government' comprising both specific state apparatuses and a particular form of *savoir*; and as both process and end-point of the transformations of the

¹⁰ This claim is variously illustrated in Roberts and Good (1993).

mediaeval State of Justice and the Administrative State of absolutism (Foucault 1979). Entailing a diachronic reading of the discourse of 'the disciplines' and of the practices constituting 'discipline', 'governmentality' was intended to accommodate the strategies of emergent actors. Although subjects may have been constructed through discipline and the disciplines, recognition of their individuality became both a precondition for and a constraint on the exercise of power. That was not simply a matter of formal rule. Rather than being centralised, governmentality was to be sought in any form of organisation, and contemporary rulership was then to be seen less as the State's domination of society than as the governmentalisation of the State:

governing people is not a way to force people to do what the governor wants; it is always a versatile equilibrium, with complementarity and conflicts between techniques which assure coercion and processes through which the self is constructed or modified by himself (Foucault 1980: 202).

Stress on interaction within and between those techniques and processes would lead to a sense of 'political rationality' whereby two errors could be avoided: treatment of the State as either Nietzsche's *monstre froid* or, in a theme of the left, as a functionally privileged position to be taken over. Economic *savoir* and control by apparatuses of security might be pervasive, but the liberal individuality co-emergent with them made any final 'discipline' impossible. Rather than a pathos of Weberian rationalisation, governmentality is an "organization of elective affinity" (van Krieken 1996: 203), and so suggests that interfusion of nature and culture which Weber took from Goethe.

Among the enrolments of this work,¹¹ Rose and Miller's elaboration of a tension between 'governmentality' and 'discipline' in the welfare state is especially relevant, for it allows a new link to be drawn with 'disciplinarity'. The 'discipline' of Foucault's earlier work remained salient in it, since the welfare state could be seen as:

one formula for recoding, along a number of different dimensions, the relations between the political field and the management of economic and social affairs, in which the authority of truth, and of experts as those who can speak and enact truth, was to be accorded a new role (Rose 1993: 293).

That 'one formula' was not a Taylorite 'one best way'. The governmentality by which managerial discipline was fused with claims from the disciplines implied a "diversity

¹¹ See, for example, Burchall *et al.* (1991).

of powers and knowledges entailed in rendering fields practicable and amenable to intervention" (Miller and Rose 1990: 3). Alignment of political rationalities in that diversity was never straightforward, for knowledges proposed as authoritative were vulnerable at every site where policy was planned, enacted and evaluated. Since those rationalities were always effected through material microtechnologies, the study of governmentality required attention to what Rose and Miller (1992: 183) called the:

humble and mundane mechanisms by which authorities seek to instantiate government: techniques of notation, computation and calculation; procedures of examination and assessment; the invention of devices such as surveys and presentational forms such as tables; the standardisation of systems for training and the inculcation of habits; the inauguration of professional specialisms and vocabularies; building designs and architectural forms - the list is heterogeneous and in principle unlimited.

The success of any policy resulting from such ensembles could not be assured. Open to unplanned consequences, to adaptation to their own ends by those involved in them, or to the lack of sustaining resources, each practice was always already unstable. Whatever governmental outcome was achieved was sure to be at odds with the rationale of the policy. In sum, the analyst of the welfare state must expect confusion, if not outright failure, rather than an overarching power of the state. Given the gap between the imaginary of political rationality and enactments of it, Miller and Rose (1990: 10) then distinguished 'governmentality', as 'eternally optimistic', from 'government', as a 'congenitally failing operation'.

Malpas and Wickham (1996) have similarly treated sociology itself as an effect of governmentality. Pointing to sociologists' assumption of and focus on successful outcomes, to the imbrication of the discipline in reformism, to the incompleteness of governance, and to the inevitability of setbacks in both governmental and disciplinary projects, they too have followed Foucault in their call for a 'sociology of failure':

governance [tends] to fail insofar as its projects will always be subject to interference from other governing practices as well as resistance from within the practice. Such resistance and interference is not accidental to governance, but intrinsic to it (Malpas and Wickham 1996: 49).

Since the discipline is entangled in the field of governing practices, they concluded that sociological study cannot be more than "an always incomplete charting of particular failing practices" (Malpas and Wickham 1996: 49).

Of specific relevance here, both pairs of writers enrolled actor-network theory.

Rose and Miller's accounts involved, *inter alia*, 'network', 'disciplined bodies', and 'inscription devices', and Malpas and Wickham (1996: 41) also used "pointedly Latourian" terms to depict "outcomes that never proceed entirely according to plan". These readings of governmentality are consistent with Latour's (1991: 74-5) own linking of 'discipline' with the 'modern':

Modern discipline has reassembled, hooked together, systematized the cohort of contemporary elements to hold it together and thus to eliminate those that do not belong to the system. The attempt has failed; it has always failed. There are no longer - there never have been - anything but elements that elude the system.

These actor-network theoretical readings of Foucault - and in particular the stress in them on 'humble mechanisms', the distinction of governmentality from government, and the contrast between an optimistic imaginary and the inevitability of failure - allow a recasting of 'discipline' in which his neo-Mannheimian bridging of civilisation and culture, or his discursive re-suturing of the epistemological and political senses of disciplinary representation, can be used without his globalising tyranny.¹²

Disciplinary discourse: Reprise

If 'disciplinarity' is to 'discipline' as 'governmentality' is to 'government', then a discursive treatment allows the disciplinary to be reconstructed. But this requires one further element. When Foucault (1976a: 139) revised "the procedures of power that characterized the disciplines: an anatomo-politics of the human body", he did not elaborate the collectively lived quality of that politics. Geertz's account of disciplines as ethnographically accessible suggests a means by which the complementary relation of disciplinary science and disciplinary discourse can in turn be complemented.

DISCIPLINARY CULTURE

Latour and Woolgar (1979) and Knorr-Cetina (1981) showed in 'laboratory studies' that

¹² Alternative genealogies are always possible. Baldamus (1972; 1976; 1977), for example, a former student of Mannheim, derived a 'sociology of error' from readings of, among others, Fleck, Kuhn and Merton; this included a discussion of dichotomies and trichotomies. Or, March and his colleagues (e.g. Cohen *et al.* 1972; March and Olson 1983) drew a similar account, of optimistic imaginaries and practical failures in government, from American organisational analysis.

the production of scientific knowledge is anthropologically accessible, and Geertz has made similar claims for 'disciplines' in general. On the assumption that:

if you want to understand what a science is, you should look in the first instance not at its theories or findings, and certainly not at what its apologists say about it; you should look at what the practitioners of it do (Geertz 1973: 5),

he has treated 'disciplinary cultures' ethnographically (or at least programmatically called for them to be so treated). Even though both his account and Becher's (e.g. 1989) potentially promising enrolment of it will be seen to entail familiar foreclosures, his emphasis on the way that disciplines are collectively lived does allow a bridging of civilisation and culture complementary to the discursive.

Geertz on discipline

Geertz associated his work with the sociologies of knowledge in general and with Kuhn's in particular, he was one of Parsons' students, and he enrolled Foucault. But where Parsons' and Foucault's uses of Weberian rationality have been stressed, Geertz (1973: 5) drew on Weberian 'meaning', in holding that "that man is an animal suspended in webs of significance he himself has spun". To trace the meanings of formal knowledge, Geertz (1983a) proposed that an ethnographic study of disciplines was possible through a fusion of his own anthropology with Kuhn's 'disciplinary matrix' and Foucault's relativism. Since he said elsewhere that he agreed with a Foucauldian argument "save for its premises, its conclusions, and its cast of mind" (Geertz 1988: 7), the last was a guarded reading. It was Foucault the crosser of disciplinary boundaries he enrolled rather than Foucault on discipline or on reading the social as textual. For this last, he held that Ricoeur's (1971) treatment of 'inscription' offered a solution to methodological dilemmas in social analysis. The "unpacking of performed meaning" or the "practical epistemology" which Geertz (1983a: 29) saw as common to Foucault, Kuhn, and his own reading of Ricoeur entailed ethnographies of disciplinary matrices. These could be variously grounded. Geertz (1983b: 155) held that disciplines:

are more than just intellectual coigns of vantage, but are ways of being in the world, to invoke a Heideggerian formula, forms of life, to use a Wittgensteinian, or varieties of

noetic experience, to adapt a Jamesian.¹³

So to belong to a discipline is to live a pervasively definitive frame. Especially in such fields - 'intellectual villages' - as mathematical specialities, where most practitioners throughout the world might know most others personally, "the interaction, indeed the Durkheimian solidarity among them would make a Zulu proud" (Geertz 1983b: 157). If with less intensity, the same should be expected of any discipline.

Study of a discipline is then a form of textual ethnography, entailing the same "puzzles of translation" (Geertz 1983b: 151) as in any cultural anthropology. Among the methods Geertz suggested for it, one was doubly textual: attention to "the vocabularies in which the various disciplines talk about themselves to themselves" (Geertz 1983b: 157). Another, taken from anthropological fieldwork, was the use of 'convergent data', by which Geertz (1983b: 157) meant data collected opportunistically in the hope that they "shed light on one another for the simple reason that the individuals they are descriptions, measures, or observations of are directly involved in one another's lives". Finally, and as one means of ordering such always chaotic data, the 'life-cycle' of disciplined practitioners might be studied. This too was taken over from fieldwork:

Passage rites, age and sex role definitions, intergenerational bonds (parent/child, master/apprentice) have been important in ethnographic analysis because, marking states and relationships almost everyone experiences, they have seemed to prove at least reasonably fixed points in the swirl of our material (Geertz 1983b: 158).

Since that 'swirl' includes informalities which cannot easily be reconciled with the formalities of disciplinary science and disciplinary discourse, Geertz's ethnographic focus on discipline as lived and the lived study of it allows an alternative view of the circularities of knowledge. But ethnography too is susceptible to foreclosure.

Latour (1991: 100-1) has spoken of anthropologists' 'pusillanimity' when they 'come home from the tropics', or return from the Geertzian 'margins' of other cultures; distinctions such as that between nature and culture, irrelevant among the 'swirl' of the exotic, are restored in analyses of the familiar. That shift is evident in Geertz's work. It appears, for example, in his use of 'Durkheimian solidarity', for whereas Durkheim

¹³ Since Ricoeur derived his model from readings of Dilthey, Weber, Heidegger and Austin, and has published elsewhere a reading of Mannheim (Ricoeur 1986), the various translations here cannot easily be separated. Mannheim's enrolment of the first three, Kuhn's of Wittgenstein, and the pragmatic strand in the sociologies of knowledge, evidence performative circularities.

(1893) had located social cohesion in the tension between sameness and difference, Geertz's assumption of only the former glossed the emergent divisions of labour within and between disciplines; in either Parsons' or Foucault's terms, these are the conditions of disciplinary possibility. Or, Latour's (1991: 103) bridging of the 'great divide' between nature and culture, in the claim that "there are no cultures", has been partly translated in this thesis into attention to the arational achievement of the rational. The distinction between rationality and its other which Geertz tacitly denied in his 'practical epistemology', and explicitly rejected in his 'anti anti-relativism',¹⁴ reappears in his description of the study of disciplinary culture as an 'ethnography of thought'. While he had queried Kantian idealism in the sociology of knowledge, on the grounds that "cognition, emotion, motivation, perception, imagination, memory ... [are] themselves, and directly, social affairs" (Geertz 1983b: 153),¹⁵ his black-boxing of 'thought' through a distinction of its products from its processes (Geertz 1983b: 147-8) asymmetrically restores the rational as privileged. His enrolments of Kuhn and Foucault then do not include their stresses on achievement of the rational through lived exemplars and material practices. That reaffirmation of the 'great divide' recurred when he himself was enrolled.

An enrolment of Geertz

Geertz's programme for the study of disciplinary culture has not been widely adopted, and when it has been, his preemptions have been repeated. He has followed his own call in work on constitutive rhetorics in anthropology (Geertz 1988). The suggestion, if not the elaboration, of a similar approach can be found in Stinchcombe's (1975) sketch of American sociology in terms of Levi-Strauss' structuralism, in Jones' (1980) fragment on the Nacirema Tsigoloicos, or in Caporael's (1995) less satirical linkage of scientists with bands of foragers, while his cultural emphasis has been adopted in the 'new sociology of knowledge' (Swidler and Arditi 1994; McCarthy 1996). More directly, Becher (1981;

¹⁴ Geertz the disciplinary analyst was also Geertz the disciplinary apologist. In defending 'anti anti-relativism', he held that "... anthropology has played, in our day, a vanguard role. We have been the first to insist on a number of things: that the world does not divide into the pious and the superstitious; ... that the norms of reason were not fixed in Greece..." (Geertz (1984: 277).

¹⁵ Scheler and Mannheim, of course, had made similar claims.

1987; 1989) enrolled Geertz in his comparisons of 'disciplinary cultures'.

Becher's studies were addressed to an abiding puzzle: while disciplines may be difficult to define, "people with any interest and involvement in academic affairs seem to have little difficulty in understanding what a discipline is, or in taking a confident part in discussions about borderline or dubious cases" (Becher 1989: 19). As 'communities of discourse', in King and Brownell's (1966) sense, or as cultures in Geertz's, disciplines were then to be treated as emergent achievements. The results of Becher's triangulation of a survey of the literature on disciplines, interviews with members of a range of disciplines, and examination of disciplinary texts, supplemented Geertz's 'swirl' of data: disciplines have distinctive vocabularies, resting on assumptions which are no less distinct for being fuzzy; disciplines have 'heroic myths' and 'disciplinary heroes'; the sliding use of the first person plural in disciplinary writing suggests the instability of disciplinary boundaries.

But a difficulty recurs in these accounts. Although Becher (1989: 6) located his studies in "the little-explored border zone between the sociology of knowledge and social studies of science on the one hand, and the study of higher education on the other", in practice he reinserted the epistemological warrants at issue in the former. As so often in the study of disciplinary knowledge, relativism proved a stumbling-block; like Geertz, Becher preempted the swirl of his material through a Mertonian insistence on the priority of the cognitive.¹⁶ Disciplines as lived became disciplines as *thought*.

Disciplinary culture: Reprise

Since they entail the privileging of disciplinary civilisation over disciplinary culture,

¹⁶ His work of purification was a crude version of the early critiques of Mannheim. He held, for example, that "to claim that every aspect of intellectual life can ultimately be explained in sociological terms seems to me to constitute an extreme form of reductionism, if not of disciplinary imperialism. If questions of truth and falsity owe nothing to phenomena outside the socially constructed interpretations of members of contemporary society, it would seem no more than a matter of arbitrary consensus that Newton's laws of motion were held to be valid, or that the atomic bombing of Hiroshima foreshadowed the end of the Second World War" (Becher 1989: 4-5). Despite the putative location of his work in the sociology of scientific knowledge, Becher does not appear to have been familiar with the field. His most recent reference to work in it was to Knorr-Cetina's (1982) discussion of transepistemic arenas, and in his claim to have adopted "an internalist rather than an externalist standpoint" (Becher 1989: 4) he begged precisely the question she had problematised.

both Geertz's proposal that 'disciplines' are ethnographically accessible and Becher's partial enrolment of it are more promising than directly enrollable. Like the purifying strain in Merton's disciplinary science and the tyrannously globalising moment in Foucault's disciplinary discourse, they need to be cut down to size. Such a restoration of Geertz's 'swirl' will be effected here in a modelling of the disciplinary which accommodates its pervasion of disciplinary science and disciplinary discourse.

MODELLING THE DISCIPLINARY

Like any network, a discipline is a heterogeneously engineered problematic translation chain, and the knowledge generated within it is then simultaneously real like nature, narrated like discourse, and collective like society. Those summarising themes from the sociologies of knowledge were found again in the exemplary accounts of disciplinary science, disciplinary discourse, and disciplinary culture, accounts which ranged between imbrication with the sociologies of knowledge and independent derivation. In all of them, 'discipline' was located within broader networks; this was the sense of Merton's distinction between discipline and profession, Parsons' linking of disciplines to political order by 'citizenship', Foucault's own and other Foucauldian resolutions of discipline and governmentality, or Geertz's insistence that disciplinary knowledge is always embedded. These networks were material, as in Merton's and Parsons' stresses on institutionalisation, Foucauldian analyses of practices, or Geertz's 'way of being in the world'. They displayed, too, both the characteristic work of purification (as in Parsons' 'contamination') and the counter but still typical acceptance of unpurified contingency (as in Rose and Miller's 'failing'). Beyond that link with the sociologies of knowledge, all the writers in this chapter referred to an autonomous science as a 'discipline', even if the meaning of 'science' was uncertain, and even if the repeated stress on the inter- or trans- or cross-disciplinary suggested that autonomy was just as contingent. Many took 'discipline' as discursive, as in King and Brownell's 'community of discourse', Foucault's archaeology and genealogy, or Geertz's and Becher's attention to the details of disciplinary language. All, finally, treated discipline as processually collective, as in Merton's stress on the

formation of disciplinary identities, Foucault's on disciplinary normalisation, or Geertz's on disciplinary generations. That amounts to agreement, in broad and in detail, within and across each ordering moments, between studies of discipline and sociologies of knowledge. Both are rationalisations of the irrational

To sharpen that agreement, the rhetorical move from Chapter 2 will be repeated. On assumptions that the disciplinary is an effect of networks, and that any network entails conditions of possibility, maintenance, and disparate practices, the disciplinary can be modelled as a trinitarian effect of 'disciplinarity', 'disciplining' and 'discipline'.¹⁷ Intended to subsume the elements noted throughout this chapter in a way which allows suspension of purification, these denote, respectively, an optimistic imaginary, a set of normalising practices, and a state of permanent failing.

Disciplinarity

That sense of 'disciplinarity' is adapted from the usages above. Both Heckhausen (1972) and Klein (1990) meant by it the state of being scientific, but Klein also used 'interdisciplinarity' to refer to a field of study. More reflexively, Messer-Davidow *et al.* (1993) used it to denote both the practices and possibility conditions of discipline and, again, a distinct field of study. That layered reflexivity is retained in this enrolment, while 'possibility conditions' and 'practices' are separated by analogy with Miller and Rose's (1990) reading of Foucault; 'disciplinarity' is to 'discipline' as 'governmentality' is to 'government', and the two already processual states are linked by processes of 'disciplining'.

So 'disciplinarity' is defined as an optimistic imaginary of discipline, where 'imaginary' denotes not a subjective state, but Lacan's (e.g. 1949) reflective sense of that which forms the subjects who live it. 'Disciplinarity' is as materially performative as are, say, Sorel's (1907) 'myth', Mannheim's (1929c) 'utopia', Mead's (1934) 'generalised other', Althusser's (1970) 'ideology', Anderson's (1983) 'imagined communities', or

¹⁷ The form, if not the detail, of this modelling is almost conventional. Althusser (1964: 183), for example, talked of "the structure of every scientific discipline" in terms of the practical, the technical, and the theoretical, or Habermas (1968) of technical, practical and emancipatory 'knowledge-constitutive interests'.

Hobsbawm's (1983) 'invented tradition'.¹⁸ Traditionally tempered and emergently renewed, it is the ensemble of those contestable possibilities which allow construction and maintenance of discipline. It is that fluidly stable repertoire - found in, say, rhetorical common sense or Scheler's 'relatively natural worldview' - through which participants in any disciplinary conflict recognise and establish the authoritative. Stability is evident in the agreement between the problematics in the previous chapter and in this. To reconcile them, and in such a way as to allow for the epistemological and political senses of representation, the already trinary 'disciplinarity' is further split into three moments: the disciplinary is 'scientific', 'useful', and 'collective'.

The 'science' of disciplinarity is an optimistic imaginary of pure knowledge, 'usefulness' of legislative/practical application, and 'collectivity' of a community of knowledge-producers. Those performative imaginaries, already seen in Mulkay's (1993) 'rhetoric of hope', recurred in all the accounts of 'discipline'. To illustrate the optimism of their interpenetrative invocation, Parsons' (1959: 547) description of sociology might be recalled; it was, he held:

universally conceived as a scientific discipline which is clearly *primarily* dedicated to the advancement and transmission of empirical knowledge in its field and *secondarily* to the communication of such knowledge to non-members and its utilization in practical affairs.

Since the claim is given explicitly and contextually as communal, since the ordinal adverbs suggest both the permanent co-presence and contestability of the scientific and useful moments of disciplinarity, and since the optimistic character of the description is evident in Parsons' 'universally' and 'clearly', that one passage epitomises what has

¹⁸ Sorel's (1907: 32) 'myths' were "not descriptions of things, but expressions of a determination to act". Mannheim's (1929c: 192) 'utopian mentality' referred to "that type of orientation which transcends reality and which at the same time breaks the bonds of the existing order". The "structure ... on which the self is built" was the response to Mead's (1934: 162) generalised other. Referring to his similar 'mirror stage', Lacan (1949: 4) talked of "the function of the *imago*, which is to establish a relation between the organism and its reality". Althusser's (1970: 153, 155) Lacanian theses on ideology were that it "represents the imaginary relationship of individuals to their conditions of existence" and that it "has a material existence". Anderson's (1983: 15) national communities were 'imagined' "because the members of even the smallest nation will never know most of their fellow-members, meet them, or even hear of them, yet in the minds of each lives the image of their communion". Hobsbawm's (1983: 1) 'invented tradition' denoted "a set of practices, normally governed by overtly or tacitly accepted rules and of a ritual or symbolic nature, which seek to inculcate certain values and norms of behaviour by repetition, which automatically implies continuity with the past".

been found more generally. 'Science', 'usefulness', and 'collectivity' are held as distinct under work of purification and as co-emergent when that work is suspended, but they recur in both types of accounts: the three moments of disciplinarity represent abiding problematics. They also serve as resources for 'disciplining'.

Disciplining

'Disciplining' refers to the multiplex maintenance and renewal of discipline. Following Foucault's (1975: 183) stress on the variety of the microtechnologies by which penalty "compares, differentiates, hierarchizes, homogenizes, excludes... *normalizes*", or Law's (1986b) on the heterogeneous engineering of networks of documents, devices and drilled bodies, 'disciplining' here denotes all the textual, institutional and embodied invocations of the three moments of disciplinarity. It refers to their mobilisation as performative imaginaries. But where Foucault (1970a: 15) held that the rule-bound conditions of discipline form an anonymous system, freely available to whoever wishes to use them, 'disciplining' is used here more restrictedly. It is a particularly situated learning - to read, write and be in a particular way in and for a particular collectivity - and a particularly situated normalising of that learning. *Mutatis mutandis*, that is the sense which Merton (1971; Thackray and Merton 1972) imputed to the accepted criteria of evidence and the formation of identities within a discipline, or which Geertz (1983b) stressed in his Heideggerian 'way of being in the world'. Such disciplining is never, can never be, complete. The invoking and enacting of disciplinarity constrains but does not determine. The contestability of 'science', 'usefulness' and 'collectivity', separately and in their interaction, does not allow closure.

Discipline

The 'discipline' which emerges agonistically from and in turn agonistically sustains disciplinarity through this disciplining cannot be guaranteed. As Miller and Rose (1990) said of government, and Malpas and Wickham (1996) of sociology, a discipline as practised is a constantly failing operation. The documents constitutive of it may be read in unexpected ways, its devices turned to other purposes, or drilled bodies cease to be

docile, and all with legitimation from the very repertoires of disciplinarity. As a problematic translation chain, a discipline is always susceptible to dispersion at any of its component links. If disciplinarity is the material dreaming of foundations, and disciplining is their material invocation, discipline is the material refutation of their realisation. It is a Sisyphean effect. As seen, say, in Parsons' (1959: 548) trope of sociology as a 'young science', the limits of disciplining and the failing of discipline become part of the optimism of disciplinarity. In a version of Kuhn's 'essential tension' between tradition and innovation, this entails a fusion of collective amnesia and collective memory. Past failures are included in the imagining of 'science', 'usefulness' and 'collectivity' through a discounting of failures to come. Against the actual practice of discipline, disciplinarity is renewed always in the hope that now, at last, this time, real progress will be made.

Modelling the disciplinary: Reprise

Dual objectives were set for this modelling of the disciplinary from accounts of 'discipline'. As metadisciplinary, it is one step in the argument that formal sociologies of knowledge are representations of discipline, and, as subdisciplinary, it is one step toward study of informal representations.

Read as concrete exemplars, the accounts of 'discipline' were seen as isomorphic with the sociologies of knowledge, for the problematics of existential determination recurred within and across the treatments of disciplinary science, disciplinary discourse and disciplinary culture. Linked unevenly to the sociologies of knowledge, studies of discipline repeated both the work of purification in them and the potential for a lifting of it. That is one warrant for a repetition of the rhetorical move from the previous chapter. Just as the homology between Mannheim's, Kuhn's and the actor-network theorists' work was used for a trinitarian model of knowledge and its study, so the disciplinary can be read triadically. 'Disciplinarity', 'disciplining' and 'discipline' do not map directly onto, say, Mannheim's reading of liberalism, socialism and conservatism, but the same sense of interaction pervades both trinaries.

A more specific link is evident when disciplinarity is itself taken as a triadic

effect, of 'science', 'usefulness' and 'collectivity'. As already suggested etymologically, the contested interfusion of those imaginaries has the same genealogy as the imagined tradition of the sociology of knowledge. When Comte (1830-42 I: 12, 16) claimed that positivism offered "the only solid basis for that Social Reorganization which must succeed the critical condition in which the most civilized nations are now living" and offered the slogan of *Savoir pour prévoir, pour pouvoir*, or when Marx (1845: 423) held that "[t]he philosophers have only *interpreted* the world, in various ways; the point is to *change* it", each was drawing on what was long-established. As Merton (1938: 228-232) stressed, Bacon, Hobbes, and Locke all linked 'science' and 'usefulness', where the latter implied both practical need and social order; it was an axiom in Vico's (1744: 61) 'new science' that "[t]o be useful to the human race philosophy must raise and direct weak and fallen man, not rend his nature or abandon him in his corruption"; and all of these could look to Plato's golden dreaming. Plato's Republic, Bacon's (1627) Salomon's House, or Comte's Parliament are exemplars of a recurrently invoked imaginary, up to recent sociologies of scientific knowledge.¹⁹ The continuity from Bacon's (1602: 383) claim that "[k]nowledge and human power are synonymous" to Foucault's disciplinary 'power/ knowledge' is the continuity in the fusion of epistemological and political senses of representation given in rhetoric and the sociologies of knowledge.²⁰

Because of that rhetorical grounding, the use of 'model' here differs from the usual sociological sense. Where analytical models entail relations between mutually exclusive categories, each of the triads of disciplinarity, disciplining and discipline, science, usefulness and collectivity, and documents, devices, and docile bodies is, by definition, always already interactive: each moment in each is an effect of and a precondition for each of the other two. That breach of analytical logic follows from the emergent and enthymemic rationality which remains once the purification in *a priori* privileging of the epistemological is suspended. The logic of the model emerges in its

¹⁹ See, for example, the special issue of *Social Studies of Science*, introduced by Richards and Ashmore (1996). cf. Mulkay (1991: ix): "I have come to see sociology's ultimate task, not as that of reporting neutrally the facts about an objective social world, but as that of engaging actively in the world in order to create the possibility of alternative forms of social life".

²⁰ "No writer was more enthusiastic or more influential than Francis Bacon, lord chancellor of England and court counselor to Queen Elizabeth I and King James I, in making a joint case for the reform of learning and the expansion of state power" (Shapin 1996: 127).

derivation and in its use.

So at a metadisciplinary scale the modelling of the disciplinary into nested triads allows the sociologies of knowledge to be summarised. If 'discipline' epitomises sociology's location in what is studied within it, then the isomorphism between it and the study of knowledge, the effects in both of the same historical shifts, and the continuities in both across those discontinuities, mean that it can serve as a focal trope of the formal. It also opens one means for subdisciplinary study of informally organised knowledge.

A focus on the repertoires of disciplinarity, the practices of disciplining, and the failing of discipline is consistent with Mannheim's attention to 'knowledge', Kuhn's to the 'disciplinary matrix', and the actor-network theorists' to 'translation chains'. Wide applicability follows from the self-limiting in the model's form and content, for trinitarian ordering allows both epistemological purification and the 'globalising tyranny' of Foucault's use of 'discipline' as an overarching trope to be avoided. As disciplinary effects themselves, the model and its use are subject to the dispersion and contingency included in it.

After a note on Gans' {1988} account of 'discipline', those subdisciplinary openings will be elaborated in the following chapter, on disciplinary methods.

CASE 3 : GANS' DISCIPLINE

Of the sixty presidents, Gans {1988} most problematised 'the discipline' of sociology. Since he also most explicitly linked his address to the sociology of knowledge, his talk on 'Sociology in America: The Discipline and the Public' allows a preliminary test of the model of disciplinarity-disciplining-discipline, and so another preview of the relation between the formal and folk axes of the thesis. As expected from the note on Wirth {1947} and Gamson {1994}, Gans foreclosed on formal reflexivity, but his address can be read as a folk sociology of knowledge,

As a first move towards a needed renewal of the discipline, Gans called for research into 'Sociology in America', and his sketch of 'a set of studies in the sociology of knowledge that tries to understand where we are coming from and going and how we are tied to the main structures and hierarchies of American society' was straight from the sub-/metadiscipline. He did not mention Mannheim, but when he asked whether 'we somehow ... represent particular interest groups, or falling, not to mention rising, classes', his terms were familiar.¹ His noting of the rhetoric of disciplinary knowledge, and his querying of 'our identity as sociologists in an era of ever greater specialization of fields and sub-fields within the discipline', could similarly be taken from any number of more recent sociologies of knowledge. Presidential examination of the discipline then appears commensurate with sub-/metadisciplinary study. But Gans also in effect denied that equation, for he warned later that:

Whether the study of Sociology in America involves basic, applied, or policy oriented research, we will, in effect, be studying ourselves. I need not list the dangers of a disciplinary wide self-study, and in a utopian world, another social science would study us while we study yet a third. However, in this world, we have to do the needed studies and we have to learn how to deal with the likely conflicts of interest. An essential ingredient for self-study is the right mixture of deliberate and systematic reflexivity and an equally deliberate and systematic distancing.²

While the call for simultaneous reflexivity and distance is familiar, the appearance of

¹ Gans studied the sociology of knowledge with Wirth {1947}. He recalled elsewhere his excitement on first encountering *Ideology and Utopia* (Gans 1990: 439).

² He used 'reflexivity' in Gouldner's (1970) sense.

'utopia' is jarring. If Gans used it in Mannheim's sense of a performative imaginary, then his debarment in principle of a sociological examination of sociological practice is counter to Mannheim's project. Likewise, even though he was not unfamiliar with post-Kuhnian and rhetorical work in the sociologies of knowledge, his finding it 'scandalous [that] funding agencies with public mandates or tax exemptions nonetheless base their grant policies on the power balance inside disciplines' suggests that he maintained the separation of epistemological and political problematised in them.³ So, whether it is occasioned or is particular to Gans, what at first appears a consonance between sub-/metadisciplinary study and presidential addresses is in fact denied.

But if Gans' standard avoidance of 'Mannheim's paradox' then leads to the standard sociological oddity of a reluctance to sociologise, the tacit sociology of knowledge in his address is consistent with the model of disciplinarity-disciplining-discipline derived from formal studies.

'Disciplinarity' was defined as an imaginary of discipline, and 'discipline' as an actual effect of it, with the two linked through 'disciplining'. 'Disciplinarity' was further disaggregated into moments of 'science', 'usefulness' and 'collectivity', and 'discipline' into assemblages of 'documents, devices, and docile bodies'. The three moments are interwoven throughout the address, in broad and in detail.

Gans certainly assumed scientificity. Even if sociology was 'the residual social science', it was 'a *scientific* discipline, in which research problems ought to determine the methods'. To be sure, what was to count as 'science' was contested. Gans held, for example, that orthodox dismissal of qualitative methods in the name of scientificity was misplaced, since 'in fact intensive interviewing and fieldwork are generally more scientific because the researchers get to know closely the people and social structures they are studying'. He maintained, too, that sociology should be useful, and in terms familiar from Comte, Marx and the traditions they had evoked. Part of his rationale for study of the discipline was that '[w]e need to know whom we help and whom we injure and damage ... so that we can figure out what we should be doing and not doing in behalf

³ As "a participant-observer and essayist in a discipline whose dominant research tradition is highly quantitative" (Gans 1990: 446), he wrote from what he considered a minority position.

of a better society, however "better" may be defined'. Focussing on 'what we still need to do to serve the lay public and the institutions in which it is involved', he devoted much of his address to possible means by which the contributions of sociology could be maximised. As already suggested by his use of the first person plural, the third moment of disciplinarity, 'collectivity', pervaded the other two. Gans also made this explicit. Holding (in familiar Mertonian terms) that 'our identity as sociologists' was 'social as well as intellectual', he enjoined his audience 'to identify intellectual cores that are common to many of us', and 'to look at social mechanisms that can contribute to our being and feeling a part of a single discipline'. This in turn suggests the second moment of the disciplinary, 'disciplining'.

Disciplinarity is enacted and recast through disciplining invocations of science, usefulness and collectivity. This process is clear in Gans' justification for his address - '[i]nstead of writing a paper that might have been relevant to only some colleagues ... I chose a topic in which all of us are or should be interested, the discipline' - and is reiterated throughout it. Drawn as it is from collective understandings, that use of 'should' is characteristic of normative disciplining. Gans linked it most obviously to 'usefulness'. On the grounds that '[m]aintaining some relationship with the American public is part of our responsibility as members of society and as recipients of its funds', for example, he claimed that 'we play a smaller part in the country's intellectual life than we should'; or he held that 'our identity as sociologists ... should concern all of us, to further our own well being and to help us make our case for the desirability of sociological knowledge to the lay public'. But when he charged both quantitative and qualitative researchers with 'mindlessness' he extended his policing from 'usefulness' to 'science. Again, invocations of 'collectivity' pervade both of these.

Gans started from the sense of failure expected of 'discipline', since his context was a perceived disciplinary fragmentation. Indeed, if 'reintegration is neither likely nor desirable in a pluralistic discipline' it was not clear 'to what extent we can remain a single discipline'.⁴ Failure was evident in documents (the irrelevance to most

⁴ Of course, the extent to which sociology had ever been integrated is uncertain. Gans (1990: 449), for example, claimed to "remember virtually sneaking into the campus bookstore for my copy of Merton's *Social Theory and Social Structure* because the rivalry between the Columbia and Chicago sociology departments discouraged undue interest in Columbia authors".

sociologists of papers in the leading journals), in devices (inadequate organisation), and in the disciplining of bodies ('the ways we still often mistreat graduate students and part-time instructors', or 'the ever-expanding star system and the treatment of some colleagues as celebrities'). Gans might be said to have exemplified this failure, for it is a peculiarity of his address that he did not define his central term. Tacitly granting 'failure', he did lodge a quasi-definition:

Before I start I must define the term 'we'. I use it broadly, referring to 'we the discipline' and 'we the collectivity', knowing all the while that the discipline is highly diverse while the collectivity is far from a functioning sociopolitical entity. 'We' is therefore mainly a shorthand about how numbers of us act or how we should all act, but I must apologize to the practitioners that my 'we' is mostly the academic discipline and collectivity.

But while the 'science' and 'usefulness' of sociological disciplinarity were implied in the explicit 'collectivity, the distinctiveness of 'the discipline' remained unclear. On the one hand, sociology was 'the residual social science', and Gans urged the breaching of the collectivity achieved in it: 'we ought ... to be moving deliberately across the accepted or imagined boundaries of sociology', for:

Disciplinary boundaries in the social sciences are arbitrary anyway, and they should be crossed freely, preferably for substantive, not imperialistic, reasons. We should act similarly toward boundaries beyond the social sciences and take a greater interest in the humanities.

On the other, if such arbitrariness meant that other disciplines faced the same tensions over internal coherence and external relevance as found in sociology, it also gave grounds for distinctiveness to be asserted: 'the imperfections of the other social sciences do not excuse ours - and besides, we should be the first to overcome them, thus leading the way for the others'. So in this apparent contradiction over what 'the discipline' entails, the address enacts its 'congenitally failing' character.

Perhaps Gans did not need to define 'the discipline', for as he said elsewhere (Gans 1990: 446), the term was a taken-for-granted descriptor. In that case, the address might not be bad formal sociology of knowledge, but a successful presentation of the folk version of it. That is the tension to be examined throughout the thesis. For the moment, the appearance in Gans' address of the 'disciplinarity', 'disciplining' and 'discipline' derived from the sub-/metadiscipline confirms the potential for an antimetabolic

relation between formal and folk accounts. That relation is formalised in the next chapter, on disciplinary methods.

CHAPTER 4 : DISCIPLINARY METHODS

In the first phases of the argument that sociologies of knowledge are representations of discipline, exemplars of those sociologies were patterned as responses to 'Mannheim's paradox', and exemplary accounts of 'discipline' were shown to be isomorphic with them. The effects of sociology's location in what is studied within it recur in methods used for the study of disciplinary knowledge. Any ordering requires a method, but to rely on abstract methodological prescriptions is to interrupt sociology's circularity, and so to repeat the characteristic separation of the political from the epistemological. In this third step of the argument, circularity will be resolved through a representation of disciplinary methods, and so of disciplinary rhetorics in the sociologies of knowledge, as processes and effects of 'fractal ordering'.

This chapter is structured by the emergent architectonic of the previous two. Latour's (1987) prime methodological injunction - 'follow the actors' - has been adopted from the start, where the actors were writers of exemplary texts. So too has Law's (1994: 107, emphasis removed) 'intuition', that:

it is plausible to go out and look for fairly coherent and large scale ordering patterns in the networks of the social. It is ... plausible to look for orderings which (to the extent they are performed) generate, define, and interrelate elements in relatively coherent ways.

One such 'fairly coherent' pattern was identified in an homology between Mannheim's, Kuhn's and Latour's accounts of knowledge, as summarised in Figure 2.2 (p. 60). The reading of the disciplinary as scientific, discursive and cultural was derived from that homology, and the same move is repeated here. Exemplary accounts of disciplinary methods - chosen for applicability to presidential addresses - will be ordered under scientific/quantitative, rhetorical/discursive, and ethnographic/phenomenological moments. Although this then resembles the 'triangulation' found in texts on methods (e.g. Denzin 1970; Babbie 1992), the fixity of the trope taken from surveying is

precluded.¹ Instead, the 'relatively coherent' ordering pattern found in and across the three clusterings of methods is constitutively unstable; regularity appears in recurrence of the non-arbitrarily contingent. The 'modesty' Law claimed for his 'modes of ordering' is no virtue; it cannot be otherwise.

This does not rule out an attempt at 'large-scale ordering'. Just as Chapters 2 and 3 were at once meta- and subdisciplinary, this account of disciplinary methods has dual aims: as metadisciplinary topic, methods will be used to show an isomorphism between existential determination of knowledge, discipline as a focus of it, and means for its study; as subdisciplinary resource, they will be enrolled toward comparison of formal and informal sociologies of sociological knowledge. But just as the formal and informal will be shown as interpenetrative throughout the rest of the thesis, the meta- and subdisciplinary are always co-performative. To maintain the distinction would be to continue the preemption of teratologies in the field, and so it will now be collapsed.

The term 'fractal ordering' will be introduced, as a Kuhnian 'preferred analogy' for the argument of this first part of the thesis. It allows the uncertainties in the sociologies of knowledge, in accounts of 'discipline' and in disciplinary methods to be reconciled with the 'fairly coherent' patterns within and across them. The meta- and subdisciplinary will be seen as mutually fractal, and the trope will then be used to specify the sense in which sociologies of knowledge are representations of 'discipline'.

REAL LIKE NATURE : QUANTITATIVE ANALYSIS

To use Scheler's terms, sociologies of knowledge are defined by attempts to link the 'ideal factors' of knowledge and the 'real factors' of the conditions of its production, and 'science' is a persistent imaginary in them. As Mannheim (1925: 76) said, the best part of positivism was its metaphysics. That is evident in the first element in each triad in

Figure 2.2. To show an order in it, five methods/approaches for the 'scientific', or

¹ e.g. Denzin's (1970: 26, emphasis added) 'logic of triangulation' led to the recommendation that multiple methods be used, since "[u]nfortunately no single method ever adequately solves the problem of rival causal factors". cf. Smith (1996: 43): "Triangulation is the mapmaker's method. If you know the position and height of any two places and you see a third, you can work out its position and height. That's what maps are, invisible triangles".

quantitatively empiricist, study of knowledge have been selected. The first three refer to 'real factors', or the locational attributes of disciplinary writers. Disaggregation of Mannheim's 'problem of generations' into age-, period-, and cohort-effects allows study of the life-cycle and historical position of disciplinary figures. Feminist writers have held that knowledge is gendered. Debates over 'discipline' and 'profession' point to the salience of disciplinary writers' organisational involvement. The other two, content- and citation-analyses, concern 'ideal factors', or characteristics of disciplinary writing.

The patterning of these five, both singly and in their possible interrelation, ends in a regularity of uncertainty, which arises from the difficulty of mathematising the relation between 'real' and 'ideal'. Whether ideal factors are switchmen on the tracks of the real, in Weber's (1915: 280) image, or whether real factors open and close the sluices of the spiritual stream, as Scheler (1925: 54) held, the showing of a positive relation between real and ideal requires well-defined independent and dependent variables. Metadisciplinary patterning derives from intractable problems in each of the five factors. That in turn constrains subdisciplinary use of the methods required by treatment of knowledge as real like nature.

Real factors - Age and cohort-analysis

'Age' as a real factor exemplifies the patterning to be found throughout this section and this chapter as a whole, for rather than being definable as a free-standing variable it will be seen as itself involving the real, discursive and collective effects from the previous two chapters. The 'age' or 'generation' of knowledge-producers was variously noted there. Mannheim (1927b) took generational location as a supplement to class; his account has been reworked in Mertonian terms (e.g. Riley *et al.* 1972); Merton (1984) has proposed a Durkheimian 'socially expected duration' as a complement;² and Geertz (1983b) suggested that study of age-related disciplinary rites of passage offered some stability in 'the swirl of our material'. More generally, all writers on disciplinary knowledge, even those claiming a timeless universalism, have stressed the need for

² 'Socially expected durations' are "socially prescribed or collectively patterned expectations about temporal durations imbedded in social structures of various kinds" (Merton 1984: 265-6). Found in 'anticipatory social behaviour', they may be formalized, as in limits on a term in office, but more commonly derive from ultimately inarticulable normative assumptions.

historical specificity in analysis. Study of this factor is then marked by the tension between generational particularity and transgenerational continuity.

In political sociology, the generational effects Mannheim suggested have been studied through the relation between ageing and increasing conservatism (e.g. Lipset 1960; Lipset and Ladd 1971), or through shifts in 'values' (e.g. Inglehart 1977). Pointing to the ambiguity of Mannheim's original use of 'generation' and its entanglement in folk knowledge, however, other writers have held that since it both denotes literal descent and connotes a range of metaphors it is at once too limited and too polysemic to be useful (e.g. Riley *et al.* 1972; Kertzer 1983). They have replaced 'generation' as an analytical unit with 'cohort', "the aggregate of individuals (within some population definition) who experienced the same event within the same time interval" (Ryder 1965: 845); as a 'birth cohort', 'generation' is a special case of entry to a social system. They did retain Mannheim's sense, in treating a cohort less as homogeneous than as tending toward a characteristic distribution of heterogeneity. On that basis, they differentiated the effects of 'age' into three dynamics: 'age effects' refer to physical and psychological changes over the life-course; 'period effects' to the impact of events in the historical era through which a cohort passes; and 'cohort effects' to those due to membership in a cohort, under its formative definition. But while that set of distinctions was intended to improve Mannheim's analytical rigour, it confirms instead Mannheim's questioning of analysis, for the three effects are not reducible to mutually exclusive categories:

Regardless of how cohort data are examined, two kinds of effects are confounded with one another; age and cohort effects are confounded in cross-sectional data by age, age and period effects in intra-cohort trend data, and period and cohort effects in trend data for each age level (Glenn 1976: 900).

That has been widely endorsed: statistical resolution of the effects of age, cohort and period is impossible. Because of that, treatment of one 'real factor' repeats the pattern of this chapter, for age-, period-, and cohort-effects require, respectively, 'scientific', historico-narrative, and phenomenological study.

In the first sense, age has been studied in Mertonian sociology of science, albeit not widely. Zuckerman and Merton (1972: 294), for example, held that "just about any methodical research on age, age cohorts, and age structure in science would qualify, through prior default, as a 'new' direction". Since they repeatedly noted that the data

required for analysis were not available, their remarks on age as a possible mediating factor between social and cognitive structures were more hypothetical than conclusive. Only some of their suggestions appear to have been tested, and results have been ambiguous. Thus the age-related 'Matthew effect' remains an open question (Merton 1968c, 1988; Cole and Cole 1973), while Cole (1979) found no support for Zuckerman and Merton's suggestion that such codified sciences as physics are more open to significant contribution by the young than are diffuse disciplines like sociology. Cole's (1979: 977) conclusion that "age has only a minor influence on scientific performance", however, and that such effects as can be detected are filtered through the reward-system of science, is consistent with their proposal that an optimum policy on science "is apt to be developed neither by gerontocracy nor by juvenocracy but, like the community of scientists itself, by age-diversified meritocracy" (Zuckerman and Merton 1972: 337). The uncertainty remains. In treating 'age' within a survey of the sociology of science, Zuckerman (1988: 534) concluded that the "jury is still out on the interconnections between the codification of scientific knowledge and age stratification in research performance".

That applies as well to the second moment in the study of age. It is axiomatic in the sociologies of knowledge that claims to knowledge are more or less plausible under particular historical conditions, but the use of history to explain knowledge is another question. In a version of Latour's (1987) argument that neither nature nor culture can be used to explain the other, since each results from controversies involving both, meanings imputed *a priori* to historical events cannot be used to account for knowledge; meaning and knowledge are co-emergent. As, say, Hutton (1991) showed in the historiography of the French Revolution, and in a way consistent with Mannheim's historicism, the significance ascribed to historical events is always a contemporary effect. Events are not stable markers. So when Wilner (1985), for example, concluded from her survey of articles in the *American Sociological Review* that sociologists "have been supremely indifferent to significant events", she had to bracket the suffusion of the writing and reading of the articles with the taken-for-grantedness of the significantly here and now. If the coding of 'period effects' requires the glossing of these difficulties, then they must be as elusive as those of 'age effects'.

That applies again, and through the same processes, to the decisive events inducing 'cohort effects'. While students of 'collective memory' have confirmed the impact of, say, 'the Depression' or 'World War II' (e.g. Schuman and Scott 1989), their studies are limited by the invention of traditions. As Shils (1981), Hobsbawm (1983), Lowenthal (1985), Connerton (1989) and Fentress and Wickham (1992) have variously shown, while there can be no doubt that memories of the intensity required for 'cohort effects' can be identified, their state of being always under reconstruction prevents any fixed use of them.³ Even if meanings were stable, it would still be vague who belongs to a cohort under its formative definition. Mannheim's (1927b: 281) Diltheyan argument that the dispersion of 'generation units' within a generational *entelechy* results in "the non-contemporaneity of the contemporaneous" leads to one form of uncertainty, and Latour's reversal of the point to another. If all authors are our contemporaries, through their treatments of still current problematics and through their re-creation in current debates (Serres and Latour 1990: 44), then it is possible to talk of the contemporaneity of the non-contemporaneous.⁴

So while the real factor of 'age' appears necessary for disciplinary analysis, its study repeats the interpenetrative effects of the sociologies of knowledge. Further, the romantic-historical sense of interior time which Mannheim (1927b) contrasted with the quantifiable remains out of reach. Merton's (1984) 'socially expected durations' could be used to supplement both his own 'scientific' studies and Mannheim's phenomenological stress, but these too are emergent rather than being specifiable *a priori*. In sum, age-, period-, and cohort-effects may all be quantifiable, but the characteristic distributions of heterogeneity expected in them suggest more the non-arbitrarily contingent than any stable patterning. Both that limit on their use and the scalar repetition leading to it recur in studies of another 'real factor', the gender of knowledge-producers.

³ As Shils (1970: 233) said, with reference to sociology itself, "The definition of sociology by the social structure of sociology and by its corpus of works has a retroactive effect on the construction of the sociological tradition. While traditions work forward in time, the construction of a legitimacy, inspirational tradition is a movement through time in the reverse direction".

⁴ Daniel Bell (1966: 709-10) similarly reversed Mannheim's usage, in his pasquinade on both sociological language and the new left.

Real factors - Gender

The inclusion in recent sociologies of knowledge of a stress on the gendering of knowledge (e.g. Law 1994; McCarthy 1996; Lykke and Braidotti 1996) displays the tangled recursions and dispersions of the field. Scheler (1913) wrote on 'the meaning of the feminist movement', and Mannheim had counted the sex of knowledge-producers among the 'social locations' with which he supplemented 'class'. But while feminism and the sociologies of knowledge are linked by the questioning in both of the rational as universal, while both, that is, are forms of 'anti-Enlightenment critique' (Harding 1990), and while a specific connection could be traced through the work of Mannheim's student, Viola Klein (e.g. 1946),⁵ the two were developed independently until the 1970s. Even so, familiar processes recur in feminist accounts of knowledge: they have the same structure as in the sociologies of knowledge; and quantitative methods in the study of gender entail the same circular effects.

To talk only of sociology, Bernard's (1973: 781) claim that its "male bias ... to date has interfered with our knowledge-based understanding of the way our society operates" may have been widely shared, but how it was to be redressed was another question. Noting that the "very notion of feminist methodology is an elusive one", Cook and Fonow (1986: 3, 5) located 'five basic epistemological principles' in it:

- (1) the necessity of continuously and reflexively attending to the significance of gender and gender asymmetry as a basic feature of all social life, including the conduct of research; (2) the centrality of consciousness-raising as a specific methodological tool and as a general orientation or 'way of seeing'; (3) the need to challenge the norm of objectivity that assumes that the subject and object of research can be separated from one another and that personal and/or grounded experiences are unscientific; (4) concern for the ethical implications of feminist research and recognition of the exploitation of women as objects of knowledge; and (5) emphasis on the empowerment of women and transformation of patriarchal institutions through research.

Even if feminist studies are given as inter- or cross-disciplinary (e.g. Keller 1995; Ferree and Hall 1996), this account is a version of the three moments of disciplinarity: 'science' is a desideratum, knowledge is required to be useful, and the inclusion of 'consciousness raising' or the insistence on a continuity between researcher and researched give the

⁵ Klein (1946: 2) studied 'the feminine character' to "demonstrate in a limited field that scientific knowledge ... does not exist in splendid isolation, but is an organic part of a coherent cultural system". She credited Mannheim with both the impetus for her study and its grounding. He had attempted 'a sociology of women' in his lectures at Frankfurt (Kettler and Meja 1995: 124-32).

interaction of those two moments as collective. Within the scientific moment, debates over 'feminist empiricism' (acceptance of science as practised but with the male subject no longer taken as universal) versus 'feminist standpoint' (the assumption that women's social location affords a privileged view) recall the contrast between epistemology and sociologies of knowledge, and lead to similar effects (e.g. Longino 1993; Hekman 1997).

When Stacey and Thorne (1985) and Ferree and Hall (1996) spoke of a 'missing feminist revolution in sociology', they referred to a scant use of 'standpoint'. But its adoption, as in Smith's (e.g. 1987) stress on women's 'lived knowledge', has evoked the charges that it is a form of 'methodological essentialism', or that it restores the distinction between nature and culture problematised in feminism (Martin 1994). On the other hand, 'feminist empiricism' has been widely used, for while there is evidence that female sociologists disproportionately use qualitative methods, most studies of gendered effects, by women and men, have been quantitatively orthodox (MacKie 1985; Grant *et al.* 1987). But the analyses then depend on assumptions which feminists have also problematised. Rossi (1970: 11), for example, found that women were:

- 43 per cent ... of college seniors planning graduate work in sociology
- 37 per cent ... of master's candidates in graduate school
- 31 per cent ... of graduate students teaching undergraduates
- 30 per cent ... of Ph.D. candidates in graduate school
- 27 per cent ... of full-time lecturers and instructors
- 14 per cent ... of full-time assistant professors
- 9 per cent ... of full-time associate professors
- 4 per cent ... of full-time full professors
- 1 per cent ... of chairmen of graduate sociology departments
- 0 per cent ... of the 44 full professors in the five elite departments.

The figures appear damning. But their effectiveness derives from assumptions of the universal - assumptions already built into the use of quantitative method - and they are what is at issue.

So familiar circularities recur in these accounts of relativised knowledge, and give rise to familiar difficulties. Inclusion of gender as a social location is necessary in the study of disciplinary knowledge, but quantitative study of it necessarily repeats the regular irregularity found in 'age'. The same trend is found in the organisational characteristics of knowledge-producers.

Real factors - Organisation and discipline

If the networks in which knowledge and the social are co-constructed are the stronger the more they are formally organised, the organisational characteristics of knowledge-producers are 'real factors'. Actor-network theory overlaps on this with normative accounts of science, for Merton repeatedly stressed that formal knowledge is institutional, and much Mertonian sociology of science has been directed to the effects of organisation. That need not be re-covered here, and general studies of organisations are too numerous to review. Attention is restricted to a few precedents in the quantitative study of academic organisations, which are enough to show that this form of quantitative analysis leads to the familiar dispersions.

In studying academic freedom in the social sciences during 'the difficult years' of McCarthyism and the Cold War, Lazarsfeld and Thielens (1958) included office in a national professional association in their operationalisation of disciplinary 'eminence' or 'prestige'. Knowledge and organisational position were then entangled effects, since 'prestige' was correlated with 'productivity', and since successful practitioners "set the standards of conduct ... followed by the less successful [and] are likely to display most clearly the attitudes ... characteristic of their profession" (Lazarsfeld and Thielens 1958: 7). Crane (1965) drew on this work in her Mertonian/Kuhnian analysis of the stratification within science as practised at 'major' and 'minor' universities. She too took 'productivity' and 'recognition' as independent variables. Indexing the latter by the number of honours which scientists had won, and allowing for "the fact that certain scientific honors are more sought after than others" (Crane 1965: 703), she set two categories of them. In the higher, with awards such as the Nobel Prize or election to honorary societies, she included the presidency of a national professional association. Since she noted that this index "represents a particular kind of recognition and one more likely to be obtained by an 'operator' than by a 'scholar' [and so] may measure 'success' more than 'merit'" (Crane 1965: 703), she touched on a politics of knowledge. But while she suggested that 'success' and 'merit' could be disentangled this was not a distinction she developed. Similarly, when Straus and Radel (1969: 1) indexed 'power' within the American Sociological Association through a coding of offices within it, and found that it

was correlated with the extent to which office-holders' work was used, they implied but did not elaborate the interfusion of knowledge and organisational position.

These operationalisations of the organisational characteristics of knowledge-producers all gave rise to statistically significant results. But since they also required the taking for granted of what has been problematised throughout the previous two chapters, the same point has been reached as with the other 'real factors'. Their definition as variables is compromised, the regularities derived from them rest on contestable irregularities, and any use of them can then be no more than provisional. Variables drawn from 'ideal factors', or characteristics of knowledge, are just as messy.

Ideal factors - Content-analysis

As a putatively scientific "research technique for the objective, systematic and quantitative description of the manifest content of communication" (Berelson 1952: 18), content-analysis is one means of following Kuhn's, Geertz's and Becher's claim that disciplines are linguistically distinct, or Mulkay's inclusion of a 'lexicon of terms' in his repertoires. Since 'manifest content' refers minimally to the words or expressions used in a set of texts, or more generally to the 'concepts' or 'ideas' invoked, the method is suited to location of any characteristic vocabulary in disciplinary writing. Further, since the frequency of key terms allows texts first to be patterned in themselves and then to be compared with others, it is particularly suited to the study of trends (Berelson 1952: 29). But against those advantages, the limits of the method had been noted early. One pioneer in it, Lasswell (1946: 90), had claimed that:

An adequate content analysis results in a condensed description of (1) the frequency with which selected symbols have been mentioned, (2) the number of times that the mentioned symbols have been presented favourably, neutrally, or unfavourably, and (3) the number of times the presentations have been made with given degrees of intensity (intensity being measured in terms of prominence - position and emphasis - and dynamic symbol style).

While frequencies were relatively straightforward, and judgements over shifts from 'neutrality' at least contestable, 'dynamic symbol style' remained uncertain. The method was insufficiently contextual to allow for it (Pool 1959); content-analysis was "essentially a coding operation" (Babbie 1992: 317), but coding the symbolic entailed a circular imposition of common-sense (Cicourel 1964; Garfinkel 1967); the analytic was

privileged over the hermeneutic (Burgelin 1966); use of the method could not allow for the chance that the absence of an expression might be more telling than its presence (Krippendorff 1969); in short, content-analysis was not suited to the complexity and reflexivity arising from the symbolic nature of communication (Krippendorff 1980). The method is still defended despite these difficulties. Berger (1991: 24-5), for example, held that while "the interests, beliefs and maybe even the personalities of researchers are important", content-analysis still allows 'objectivity', and Babbie (1992: 313-29) likewise balanced the questionable validity of the meanings attributed to manifest content against the reliability allowed by replicable coding. But once the meaning of manifest content is problematised, the variables in content-analysis cannot be sharply specified. It must end in the familiar contingent regularity.

That does not preclude use of the method, for on the assumption that the sheer occurrence of key-words makes it "possible to identify problematic networks and study their evolution" (Callon *et al.* 1983: 196) the actor-network writers have adapted it. Use of such 'macro-terms' as 'electron spin', which "crystallize and synthesize a whole complex of sanctioned efforts, successive statements of their use, certified knowledge and recognized results" (Callon *et al.* 1983: 199), is crucial in the construction of translation chains. Since a macro-term both effects and indexes enrolments, content-analysis in this sense incorporates the contingency glossed in more empiricist usage.

A similar move and a similar result are found in citation-analysis.

Ideal factors - Citation-analysis

Citation-analysis is also a putatively scientific means for study of disciplinary texts. Where content-analysis was initiated in the study of propaganda and has been most used in analyses of the media, it is indigenous to the sociologies of knowledge, and has been applied most widely to patterns of 'influence' evidenced in scientific papers. But if the two civilisational methods were developed as applicable respectively to 'culture' and to 'civilisation', they both end in a collapsing of the distinction.

Most associated with Mertonian work, citation-analysis was one vehicle for the flourishing of the sociology of science in the 1960s and 1970s. Merton (1977: 47-54) has

talked of anticipating it, holding that beyond its informative function, citation symbolically maintained that recognition by peers of an intellectual property-right which he saw as central to the reward system of science. On that basis, the extent of citation of a text is an indicator of both its quality and the influence of its author(s). Those links have been supported; Garfield (1979: 63-4), for example, found that levels of citation were correlated with the winning of the Nobel Prize, and Cole and Cole (1971: 28) more generally that "straight citation counts are correlated with virtually every refined measure of quality" in science.

But the method has been widely queried. As even those working with Merton's normative model have found, the relation between citing and cited texts is less direct than it requires. So when his former students concluded from citation-analyses of his own work that it was usually "cited for two purposes: to confer authority on statements authors make and to identify the source of problems" (Cole and Zuckerman 1975: 158), or that since "42 percent of the articles citing Merton's paper on anomie do so in a 'ceremonial fashion'...[o]ne function that theory serves is the legitimation of the work of the utilizer" (Cole 1975: 208), they also limited the attribution of influence. Even beyond an absurdity, that to assign 'influence' within a field the analyst must be "more expert than the scientists whose communication practices he is studying" (MacRobertson and MacRobertson 1986: 168), it is agreed that not enough is known about the norms or phenomenology of citation for it to be a reliable indicator (e.g. Porter 1977; Edge 1979; Smith 1981; Cronin 1984; Liu 1993); even the initiator of the method has accepted the point (Garfield 1994: 4). As one solution, Small (1978: 339) proposed that if citations were symbols of methods or concepts, they might "serve as a kind of language system, which can be deployed with greater flexibility than ordinary language". But since meanings would then be as elusive as in content-analysis, claims to civilisational rigour are unsustainable.

Gilbert (1977) stressed the rhetorical use of citation, holding that it may justify the novelty of a position, legitimate it from prestigious sources regardless of relevance to substantive content, or display allegiances.⁶ That was one point of departure for his and

⁶ That is consistent with Becher's (1989) noting of disciplinary 'folk heroes'. Citation is a form of alignment with them.

Mulkay's (1981) discourse analysis, and the actor-network theorists have also adapted the method (e.g. Callon *et al.* 1983; Callon *et al.* 1986; Latour and Woolgar 1979). If authors' names are 'macro-terms', or if citation is one means in the translation by which actors enrol allies to set themselves as 'obligatory passage points' in a problematised area, then a tracing of references is also a tracing of networks. This use of the method reverses the Mertonian. Since translation is a de- and re-contextualisation of others' work, any cited text is modified through its placement in the context of a new network (Callon 1980: 219; Latour 1987: 35), and attributions of 'influence' are then misplaced. It is not so much that citation or quotation reveals the 'influence' of cited authors, as that these writers are in part created and re-created through enrolments. That process is not stable. Embroiled in 'contexts of citation', scientists use citation in "Byzantine political schemes" (Latour 1987: 37), the success of which hinges on the enrolment of strong allies, on the preemption of potential attack, and on the defeat of actual opponents. While the tracing of such schemes would be a matter of case-by-case detail, citations at least serve as markers of networks under construction or dispersion.

This adaptation of the method restores the uncertainty elided in conventional uses. While citations might lead to 'fairly coherent' patterns, these are qualified by Law's (1994: 107) *caveat*: "to the extent they are performed". If content-analysis is the application of civilisational methods to cultural effects and citation-analysis is reflexively civilisational, both are usable only if the distinction is collapsed.

Real like nature : Quantitative analysis: Reprise

Difficulties have been found in each method for the quantitative study of disciplinary knowledge-producers and of disciplinary knowledge. 'Real factors' cannot be operationalised sharply: both singly and as an ensemble, the effects of age, period and cohort are uncertain; accounts of knowledge as gendered repeat the civilisational/ cultural circularities of the sociologies of knowledge; coding in organisational analyses also entails a preemptive distinction of the cognitive from the social. 'Ideal factors' are no more amenable to specification, for while disciplinary texts can be ordered by content- and citation-analyses, each method requires an arbitrary foreclosure on uncertainty.

Neither set of factors then allows the sharpness required of independent and dependent variables.

The recurrence of similar difficulties across the five sets of factors amounts to a metadisciplinary ordering. When the regularities found by users of the methods are combined with the arguability of operationalisations, a new regularity is evident, a pattern of the non-arbitrarily contingent. That is expected from the sociologies of knowledge and from accounts of discipline, and the attraction of positivist metaphysics can be accommodated in those terms. Provided that quantitative methods are 'cut down to size', they can still be used. The meanings of age, period and cohort effects co-emerge with studies of them. Gendered knowledge exemplifies the construction of nature/culture. Formal organisational characteristics are always embedded in the informal. In disciplinary texts, the frequencies of usage of key terms or of references to particular writers might likewise be taken as indicative of emergent networks, or of zones of contestability, rather than as conclusive. In each case, a contingent ordering is possible, and with it the chance of establishing a relation between 'real' and 'ideal'.

Such use of methods requires that arguability be built into them. That is, on the assumption that the quantitative is already the qualitative pursued by other means (Callon *et al.* 1986: 108), the 'scientific' study of disciplinary knowledge is rhetorical.

NARRATED LIKE DISCOURSE: RHETORICAL STUDY

The textual linkage of 'real' and 'ideal' is common to the second element in each triad in Figure 2.2. In Mannheim's documentary historicism, in Kuhn's analogical models and stress on textbooks, and in Law's 'documents', Latour's 'inscription', or Callon's (1995: 51) claim that "science is a vast enterprise of writing", knowledge is always a linguistic effect. That lead will again be followed through the rhetorical tradition. An immediate connection is given in Perelman and Olbrechts-Tyteca's (1958: 513) correlate of Kuhn's 'scientific community' or 'disciplinary matrix':

All language is the language of a community, be this a community bound by biological ties, or by the practice of a common discipline or technique. The terms used, their meaning, their definition, can only be understood in the context of the habits, ways of thought, methods, external circumstances, and traditions known to [their] users...

More specifically, the epistemic arguability of rhetoric meets the uncertainty required in the quantitative methods. Now, elaboration over two-and-a-half thousand years of techniques for the presentation or study of speeches and texts has resulted in any number of means of rhetorical study. Those treated here - 'genre', 'the topics', and 'metaphor', with the last supplemented by 'intertextuality' - have all been applied to disciplinary knowledge. It will be shown, through each and through their overall effect, that the discursive methodological moment entails the same sense of the non-arbitrarily contingent as found in the quantitative, and is open to the same moderated use.

Genre

Analysts of disciplinary knowledge have increasingly stressed its presentation in such standardised forms as the textbook, the monograph, the report on research, the review article or the book review, and thus have either invoked or suggested an ancient tradition. When Geertz (1983b) read 'blurred genres' as symptomatic of the times, when Mulkay (1984) took the speeches of Nobel laureates as a distinct form, when Bazerman (1988) treated genres of scientific writing, or when Latour (1987) stressed the specificity of scientific papers, they at least tacitly evoked the rhetorical sense of 'genre'.

The classical rhetoricians distinguished between three genres: the forensic, the deliberative, and the epideictic.⁷ Forensic discourse was conceived narrowly as the oratory of the courts, and broadly as any attack on or defence of particular actions. The deliberative, concerned specifically with politics, referred generally to any attempt to affect the course of affairs. In the epideictic, or ceremonial, the orator was said to be more intent on pleasing or inspiring an audience than on persuading it, with the subject less a matter of debate than a means for the crystallisation of inchoate consensus. As the forensic and deliberative also relied on achieved consensus, and as the three genres were said to apply to the past, future and present respectively, this taxonomy is more ideal-typical than descriptive; in practice, any discourse is some combination of the three,

⁷ Aristotle (*Rhetoric* 1358b) held that three were logically necessary: "the listener must be either a spectator or a judge, and, if a judge, one either of the past or the future. The judge, then, about the future, is the assembly member, the judge about the past is the juror, and the assessor of capacity is the spectator, so there must needs be three types of rhetorical speech..."

with one or another privileged. So while the classical genres can be fitted to disciplinary writing,⁸ Perelman and Olbrechts-Tyteca's (1958: 21) comment on them is to the point: the classical typology is "of a purely practical order, whose defects and inadequacies are apparent".

But Aristotle's sense of linguistic praxis is retained in accounts of 'genre' as a "codification of discursive properties" (Todorov (1978: 18), as "typified rhetorical actions based in recurrent situations" (Miller 1984: 31), or as "ready solutions to similar appearing problems" (Bazerman 1994: 82). As action in itself and as a means of ordering action, genre is reflexively political. In neo-liberal terms, genres might be "literary *institutions*, or social contracts between a writer and a specific public, whose function is to specify the proper use of a particular cultural artifact" (Jameson 1981: 105-6), or they might be conservative, and then either be 'benign' (Fowler 1982: 36) or have "norms and interdictions ... not far behind" (Derrida 1981: 56). But whatever the emphasis in these accounts, 'genre' is a recursive and performative classification of the type required here. As documentary, in Mannheim's sense, it might be studied "much as an anthropologist sees a material artefact from an ancient civilization, as a product that has particular functions, that fits into a system of functions and other artefacts" (Miller 1994: 69).

Genres can then be included among the heterogeneous microtechnologies by which disciplines are articulated and sustained. The effects which Fleck (1935) and Kuhn (1970a) noted in the transitions between 'journal science', 'textbook science' and 'popular science' enact the consolidation of disciplinary 'facts' and disciplinary common sense. Inducing institutional responses,⁹ publication of specialised papers, monographs, textbooks, or popular articles "can be a statement of identity" (Clemens *et al.* 1995: 433). Since this is linked to "distinctive career trajectories, institutional locations, and patterns of intellectual influence", and to contrasts like "case studies versus hypothesis testing, qualitative versus quantitative, intellectual versus scientist" (Clemens *et al.*

⁸ e.g. Gross (1990: 10-11): "A scientific report is forensic because it reconstructs past science in a way most likely to support its claims; it is deliberative because it intends to direct future research; it is epideictic because it is a celebration of appropriate methods". Zimmerman (1994) has also reiterated the classical typology.

⁹ On Durkheim's understanding of deviance, for example, Collins' (1992) response to Mulkay's 'new literary forms' suggests the strength of disciplinary expectations.

1995: 434), 'genre' might suggest Merton's separation of social from cognitive identities. But given its politically emergent character, it also implies the elision of that distinction; it might constrain, but it does not determine. This institutionalisation of common sense is also found in 'the topics'.

The topics

The link between rhetorical study and the sociologies of knowledge is seen most clearly in 'the topics', or the 'places' where arguments suited to particular occasions or subjects are 'located', for when Aristotle said that in any dispute "proofs and arguments must be derived from the commonplaces" (*Rhetoric* 1355a) he grounded rationality in the mundane reasoning and situated closures of the with-world. That rhetorical reflexivity - the "priority of...finding arguments or topics, over judgement, the critique of validity in inference, is in itself a *topos* of rhetorical discipline" (Struever 1980: 69) - epitomises the tension between sociologies of knowledge and epistemology. While Aristotle's own understanding of the relation between formal and rhetorical reasoning is disputed (e.g. Owen 1968), the reading of his *Topica* in the light of the 'new rhetoric' suggests a means of disciplinary study.

The codification in the *Topica* of means of finding arguments was repeated and revised during the classical period. At one level, lists of commonplaces served simply as a technical resource for rhetors. But at another (and as the dyad of topic and resource suggests to a contemporary reader), they implied an epistemically uncertain approach to argument. As he did with 'rhetoric' more generally, Aristotle used 'topic' in a dual sense, as a method of and a subject for inquiry. In the first, it evoked his enthymemic logic, since both depended upon the appeal of the taken-for-granted. More reflexively, commonplaces include the use made of them, for like "chess-players' 'combinations', lines of argumentation are public property" (Ryle 1968: 75). Aristotle's claim that "propositions contradicting the contraries of general opinions will also pass as general opinions" (*Topica* 104a) has been read by 'new rhetoricians' as leading to that epistemic openness implied by dependence on an audience. Through the alternatives always possible in 'general opinion', a topical approach to knowledge implies rejection of "any

attempt to immobilize opinion in a theoretical armature above debate" (Struever 1980: 75), and a corresponding insistence on the conservative strain in knowledge (Billig 1989). The isomorphism of topical study with the sociologies of knowledge is evident. Mulkay's 'repertoires', for example, are recalled in a view of the topics as "repeatable and acceptable themes that deal with shared beliefs, values, and opinions ... [having] to do with situationally appropriate thoughts and actions" (Prelli 1989a: 258).

The topics have been used to study of disciplinary knowledge. Assuming that they "provide a central location for inquiry into discursive situation as civil event", Struever (1980: 69, 66) held that "the operative historical discipline is argument". Or, revisiting a commonplace in the sociologies of knowledge, Prelli (1989b: 50-1) topically reconciled Merton's (1942) original statement of the 'scientific ethos', Merton's own (e.g. Merton and Barber 1963) and Mitroff's (e.g. 1974) quasi-Newtonian extension of it - to every norm there is an equal and opposite counter-norm - and Mulkay's (e.g. 1976) ideological reading of both:

What sociologists of science have been calling the 'norms' and 'counter-norms' of science are effectively conceived as rhetorical *topoi* that index the available range of discursive strategies for establishing negative or positive audience perceptions of scientists' *ethos*.

Zagacki and Keith (1992) have read Kuhn's 'scientific revolutions' in similar terms. In each case, 'the topics' are used to stress the situated and constitutive arguability of disciplined knowledge.

Since the reading of the sociology of knowledge was rhetorical, it is hardly surprising that 'the topics' result in the characteristic pattern of disrupted regularity; they are almost definitive of contingent constraint. A rather more dispersive version of it is found in another staple of rhetorical study, metaphor.

Metaphor

Aristotle (*Rhetoric* 1410b) took metaphor as intrinsic to enthymemic logic, and whether analogy is a sub-type of it, as he held, or whether "metaphor ... consists of a condensed analogy", in Perelman's (1979: 23) version, it is crucial to both the 'new rhetoric' and the sociologies of knowledge as well. Mannheim (c1940) discussed organic images of the state, Kuhn's 'disciplinary matrix' entailed 'preferred analogies', and Mulkay included

metaphors in his 'interpretative repertoires'. The 'network' in actor-network theory was in part taken from Hesse, and she, following Black (1962), had used 'the analogical character of theories' to read "theoretical explanation as a metaphoric redescription of the domain of the explanandum" (Hesse 1974; 1980: 11).¹⁰ In terms of both rhetoric and these sociologies, metaphor is a prime means for the gaining of an audience's adherence. But while it is then constitutive of knowledge, it is subject to the same processes as found in genre and the topics; within systematic functioning and experience, it is permanently unstable.

In linking rhetoric and the sociologies of knowledge, Brown (1977: 79) attributed to metaphor "an explicitly cognitive function".¹¹ It is a 'carrying over' of meaning from one discursive domain to another (the orthodox and actor-network theoretical senses of 'translation' coincide here), in which depictions of reality are symbolically structured. Through their 'as if' quality, metaphors shape the generation of knowledge:

New metaphors, especially when elaborated into models and theories, are not merely new ways of looking at the facts, nor are they a revelation of what the facts really are. Instead, the metaphor in a fundamental way creates the facts and provides a definition of what the essential quality of an experience must be (Brown 1977: 84-5).

As in Kuhn's 'gestalt switch', metaphor recursively links perception and the accounting for its results. Since it is most effective when it evokes recognition of resemblance by the jolt of the unexpected, it depends on the achievement of sameness from difference. As in Kuhn's (1959) 'essential tension' between tradition and innovation, new knowledge is an effect of re-presentation of the taken-for-granted through the tropical possibilities of changed material conditions. If metaphors "lose vivacity as they gain veracity" (Brown 1977: 98), that is the process by which symbolically presented 'facts' become naturalised. Through the same process as the filling of the absent middle term in the enthymeme, audiences use metaphors to link knowledge and the routinely lived.

That link has a certain coherence, as in Lakoff and Johnson's (1980) claim that conceptual systems are metaphorically structured. Noting that analytical categories are expressed in spatial metaphors, and especially in those related to embodiment, they

¹⁰ Kuhn (1979a) also linked his work to Black's.

¹¹ Against the version of the dualisms from the sociologies of knowledge in Brown's treatment of rhetoric in terms of 'ideas' and 'cognition', the reading here is intended as material.

suggested that 'concepts' are experientially grounded, and, as a result, systematic. To be in the world is to be physically oriented to it, a lived experience which recurs in terms such as 'lower class' or 'high status' (cf. Schwartz 1981).¹² Of course, embodied experience of space is culturally variable. Echoing Mannheim's *Weltanschauung* or Scheler's 'with-world', Lakoff and Johnson (1980: 19) noted that is "hard to distinguish the physical from the cultural basis of a metaphor, since the choice of one physical basis from among many possible ones has to do with cultural coherence". Even so, any regularity of interpreted experience shifts that coherence to perceptual and conceptual structuring, and, if only in metaphorical expression, knowledge is then systematically organised.¹³ On that basis, what Keller (1985: 34) found in her analysis of "the sexual dialectic" in Bacon's metaphors - that formal knowledge is regularly if not altogether coherently gendered by the expectations of the times - could be generalised to any effect of embodiment.

Metaphorical analysis *has* been applied to sociological knowledge, even if Silber (1995: 326) claimed that "little has been done to analyze the metaphorical dimension of sociological theorizing". Earlier studies show the dispersive and recursive character of the discipline to be expected from its location in the disciplinary more generally. In Stein's (1963) account of 'the poetic metaphors of sociology', that location is constitutively tropical.¹⁴ When Brown (1977) wrote of the organic, textual, mechanical, dramaturgical, and ludic 'root metaphors' of sociology, he suggested the diffuseness of associations with both disciplined and popular order. Or when Levine (1995) examined the organic in particular he could point to a continuity in both linkage of the body with the body politic from at least Plato's time and reflexive treatment of it by sociologists. In Silber's own finding of an increased use of spatial metaphors, she likewise stressed sociologists' history of taking images from other disciplines.

¹² As Lakoff (1995) stressed elsewhere, this also equates perceptual order with moral order.

¹³ Lakoff and Johnson (1980) did not note the Durkheimian echoes throughout their work, but Schwartz (e.g. 1981: 5) drew the link in his structuralist sociology of knowledge: "... different vertical forms make consistent our experience of social inequality, because they convey an identical message. ... vertical categories are to be viewed not only as 'effects' of social differentiation, as Durkheim taught us, but also as codes by which social divisions themselves find meaning".

¹⁴ Stein (1963: 173-4) held that through the metaphorical structuring of their work "[t]he great classical sociologists, from Marx to Veblen, Weber, Simmel, Durkheim, and Mannheim were inveterate sociological poets".

Sociology could even be characterised by:

the tendency to take inspiration from and even relinquish authority to a specific discipline, scientific or not, external to sociology; the greater the perceived validity or prestige of another discipline, the stronger the tendency to transform some central constructs taken from it into a controlling, master metaphor (Silber 1995: 326).

Since sociologists are just as ready to critique these metaphors - Silber (1995: 325) noted, for example, attacks on Parsons' cybernetic imagery - tropical accounts of the discipline are then consistent with the sense of contingent achievement definitive of 'discipline'.

For metaphorical regularity is only ever partial and contested, images are polysemic, and interpretation is bounded but not determined. Just as the topics were defined by exigently mobilisable contraries, "readability [of metaphor] is always equivocal" (Greimas and Courtes 1979: 190). To the extent that it is metaphorically structured, disciplinary knowledge is a 'system of dispersions', in Foucault's (1969a: 37) sense, being coherent only through its contestability. That same process is evident in the generalisation of 'metaphor' to 'intertextuality'.

Intertextuality

Disciplinary knowledge is written in genres institutionalising more or less shared expectations, consists of topics more or less taken for granted, and is effective through more or less commonly grounded metaphors. As the process by which "any text is constructed as a mosaic of quotations [or] is the absorption and transformation of another" (Kristeva 1969: 66), 'intertextuality' subsumes those facets of the disciplinary with-world, for topics, genres and metaphors are all in part textual effects. Against any monological chain of causality or the binaries of structuralism, its logic derives from "*analogy and nonexclusive opposition*" (Kristeva 1969: 72), and it then implies the reconciled systematicity and dispersion stressed throughout this section:

If ... every signifying practice is a field of transpositions of various signifying systems (an intertextuality), ... [then] its 'place' of enunciation and its denoted 'object' are never single, complete and identical to themselves, but always plural, shattered, capable of being tabulated (Kristeva (1974: 111).

While 'intertextuality' is self-exemplifying - Greimas and Courtes (1979: 160) spoke of its "lack of precision" - it does allow an ordering of the always polyvalent.

Barthes used intertextuality to hold that the *vraisemblance* of classical texts

derived from any number of associations with what was assumed, known, to be natural. Interpretation is a matter of deciphering common sense in the knowledge that common sense comprises the irreconcilable. To interpret a text is then "not to give it a (more or less justified, more or less free) meaning, but on the contrary to appreciate what *plural* constitutes it" (Barthes 1970: 5). Any formal genealogy of a text is an impossibility:

The intertextual in which every text is held ... is not to be confused with some origin of the text; to try to find the 'sources', the 'influences' of a work, is to fall in with the myth of filiation; the citations which go to make up a text are anonymous, untraceable, and yet *always read*: they are quotations without inverted commas (Barthes 1971: 160).

In holding that its citational 'cacography' is accessible through connotation, Barthes linked intertextuality with rhetoric. Connotation was "a determination, a relation, an anaphora, a feature which has the power to relate itself to anterior, ulterior, or exterior mentions, to other sites of the text (or of another text)" (Barthes 1970: 8), and where the opposition of connotation and denotation in Barthes' (e.g. 1957) earlier work had left open the possibility of fixity, denotation was now treated as "the last of connotations" (Barthes 1970: 12). Meaning was then always contestable and incomplete, being achieved as much by associations not followed as by those accepted, or as much by the excluded as by the ostensibly signified. Denotation is an uncertain effect in the same way that discipline is a perennially failing realisation of disciplinarity.

But a certain systematicity remains, for like genre, or like rhetoric in general, intertextuality is praxical: "there is no other *proof* of a reading than the quality and endurance of its systematics; in other words: than its functioning" (Barthes 1970: 11). Hicks and Potter (1991) assumed that sense in their reflexive study of the sociologies of scientific knowledge, where they adapted citation-analysis through an intertextual stress on the 'citationality' of all language. Their account of the field as a recursively pragmatic achievement can be extended. Culler's (1981: 118) claim of literary criticism, that "strength comes from a strategic positioning in the discourses of a culture, and to produce a strong discourse one must be an acute analyst of intertextuality", is likewise applicable to any discipline. Both 'discipline' and 'intertextuality' are problematic translation chains, entailing the same processes of alignment and dispersion, and disciplinary knowledge is an effect of both.

Narrated like discourse: Rhetorical analysis: Reprise

The condition set in the previous section, that the uncertainty of quantitative analysis be built into its use, is always already given in each of the rhetorical themes in this: genres are contingently stable; topics are defined by arguability; metaphors are polysemic; and intertextuality leaves all networks open to breaching through appeals to the same common sense from which they are constructed. The range of affiliations of the writers noted here repeats that pattern. Since the discursive means for disciplined study of disciplinary knowledge are transdisciplinary in their sources, their alignment already enacts what is implied in the co-extension of problematic translation chains and intertextuality, that disciplinary knowledge is a "tissue of quotations drawn from the innumerable centres of culture" (Barthes 1971: 146). Here is the metadisciplinary continuity with quantitative analyses; the stability of rhetorico-discursive accounts of disciplinary knowledge is always a matter of bounded contingency.

It was to accommodate that certainty of uncertainty that Law cut Foucault's discourse down to size. On that basis, just as the quantitative, 'scientific', linkage of 'real' and 'ideal' factors was found to be amenable to restricted enrolment, so genre, the topics, metaphor and intertextuality are available for subdisciplinary use. Although Crook's (1991: 206) warning against "turning contingency into a metaphysical principle" is to the point, treatment of the contingency of those subdisciplinary openings through the metaphysical appeal of positivism appears rather different; real effects are rhetorically achieved. But disciplinary knowledge is not only quantifiable and discursive. Since it is lived, and lived collectively, those two methodological moments require supplementing with a third.

COLLECTIVE LIKE SOCIETY: TEXTUAL ETHNOGRAPHY

When Scheler and Mannheim based their sociologies of knowledge in the with-world, when Kuhn set lived exemplars in his disciplinary matrix, and when Latour spoke of a reconstructed anthropology of knowledge, each implied that quantitative analyses and discursive studies are phenomenologically collective representations of the already

phenomenologically collective. That is given in the third element in each triad in Figure 4.1. 'Textual ethnography' will be developed from it, to supplement scientific and rhetorical methods. Just as Geertz's 'disciplinary culture' was read as suffusing 'disciplinary science' and 'disciplinary discourse', 'textual ethnography' describes the lived quality of sociological inquiry.

In that sense, it is also intended to accommodate the reflexivity required in a sociology of knowledge. This study is being developed towards a doctorate, and any postgraduate work is a disciplining into a local version of a discipline. In the same way that Becker *et al.* (1961) represented the emergence of medical identities through the interaction of students with their instructors, with their work and with each other, introspection and a quasi-ethnographic attention to the melange of practices by which sociology is locally constructed and maintained are evidentiary in a sociological study of sociological knowledge. The result is a living of Bauman's (1978) 'hermeneutic spiral', and in a way which is not limited to the locally specific. Just as it is crucial to the political moment in actor-network theory that the local is at all points global and vice versa (e.g. Latour 1991), so the discipline of sociology within any one department is inseparable from global disciplinary practices and alignments. While placement of participant-observation in disciplinary microtechnologies on too formal a footing would create social impossibilities and risk regressive solipsism, its effects are nonetheless pervasive. 'Textual ethnography' is intended to acknowledge that lived reflexivity, without either pathologising it or making it the focus of the thesis.

In this section, an accord between the sociologies of knowledge and ethnography will be suggested first. It will then be argued that reading and writing disciplinary texts are ethnographic practices, and in a way which repeats the metadisciplinary pattern of bounded contingency while opening the subdisciplinary possibilities of the previous two sections.

Ethnography and sociologies of knowledge

Mannheim's (e.g. 1929b: 153-64) suggestion that intellectuals straddle the gap between their locations of origin and the relatively homogenised world of the educated applies a

fortiori to sociologies of knowledge. “[t]raditionally the concern of marginal men” (Remmling 1973: 7), the field entails both engagement with two different worlds - that is true of any sociology - and the use of each to query the fixity of the other. The sociologist of knowledge thus embodies the tensions of Simmel’s (1908b: 404) ‘stranger’:

He is not radically committed to the unique ingredients and peculiar tendencies of the group, and therefore approaches them with the specific attitude of ‘objectivity’. But objectivity does not simply involve passivity and detachment: it is a particular structure composed of distance and nearness, indifference and involvement. ... Objectivity is by no means non-participation ... but a positive and specific kind of participation.

Atkinson (1990) and Agar (1980) have used ‘the stranger’ to depict ethnographers as ‘marginal natives’ or ‘professional strangers’, and since both are forms of ‘cultural translation’, sociologies of knowledge resemble ethnographic bridgings of otherness. Schutz (1944) linked Simmel’s ‘unique ingredients and peculiar tendencies’ to Scheler’s ‘relatively natural worldview’, while Brown (1977: 53) has suggested that such characteristics of ‘the stranger’ as “[d]istance, defamiliarization, dispassionate engagement, seeing the reality behind the mask, appreciating the intrinsic qualities of the mask itself, disinterested interest, idle curiosity” be adopted not only in sociology generally but in examination of the discipline in particular. The stance of the ‘stranger’ certainly fits a student of the disciplinarity of sociology who is also being disciplined in it, for “his position in this group is determined, essentially, by the fact that he had not belonged to it from the beginning, that he imports qualities into it, which do not and cannot stem from the group itself” (Simmel 1908b: 402). Among those imported qualities are predispositions to particular problematics and particular texts.

Ethnographic reading

Since actors’ rhetorical moves were followed in accounts of the sociologies of knowledge and of discipline, a form of ethnographic reading already pervades this thesis. That process can be generalised. Since a reader approaches any set of texts with the pre-commitments Mannheim (1922) diagnosed in epistemology, the selection of which actors are to be followed has all the hermeneutic spiralling of participant-observation.

On Kuhn’s model, that use of a particular language is constitutive and symbolic of belonging to a discipline, a doctorate is a process of location by linguistic acquisition. But

since it is difficult to talk of 'sociology' in the singular, just what is to be acquired in a doctorate in it is uncertain. Any disciplinary work, variously sourced from within, variously supplemented from without, and so variously reconstructive of the tradition, has any number of potential points of articulation. Adoption of one or another is a pre-rational effect of accidents of personal biography. The reading of one set of texts might induce revulsion, and that of another an immediate sense of rightness: that *cannot* be, that *is*, the way the world is. That response has only a contingent relation to the logic of the texts, for given two equally provisional accounts, a reader can reject one while achieving coherence for the other, by granting assumptions, accepting connotations, or glossing aporias. What one reader might construe as contradictions another might take as puzzles to be solved.¹⁵ Disciplinary alignment is a matter of prejudice, describable by the dictum which Pascal attributed to God: you would not have sought me if you had not already found me.

Given the range of social locations defining any reader, reading is at once aleatory and restricted, involving enrolment of the fortuitously found and imposition of more or less arbitrary foreclosures. In a sociology of sociological knowledge, such *bricolage* has all the tension of sub- and metadisciplinary participant-observation in/ of construction of the discipline: it is subjectively objective; boundaries are both obeyed and crossed; the specific is the general. Once these ineluctabilities are accepted, a representation is the more attractive the more it can accommodate them. That is one source of the appeal of actor-network theory, and the couching of this thesis in terms of it then enacts the hermeneutic spiralling between theory and phenomena, or between the rational and the arational, definitive of ethnographic reading; a response to particular texts is justified within the orientation suggested in them, and they in turn are further enrolled on the basis of that justification.

If any reading at least tacitly requires that stance of participant-observation, representation of the results is just as much an ethnographic process.

¹⁵ Indeed, in either Kuhnian or structuralist terms - it is where the garment gapes that the body is most erotic - the existence of puzzles may be crucial to the charm of a theory.

Ethnographic writing

Ethnography is 'cultural translation' (Marcus 1994), and once 'culture' and 'nature' are reunited in an anthropology home from the tropics, so too is actor-network theory.¹⁶ The same moves are evident in both. The plausibility at issue in 'enrolment' is the attempted *vraisemblance* of ethnography (Atkinson 1990: 39), or an intertextual problematic translation chain implies the 'partial truth' found in representation of one culture to members of another (Clifford 1986). Woolgar's (1988b: 32) injunction that an "ethnography of the text must develop an understanding of text as just one element in a reader-text community" locates the process in post-Kuhnian openings of the boundaries of a 'scientific community'. Where reading is a matter of enacted prejudice, writing in these terms is a bid to generate 'community' through evocation of as wide a range of predispositions as possible. 'Translation' is the process to be both studied and attempted in a sociology of sociological knowledge.

If its limits are to be accommodated in its metadisciplinary moment, then the generic difficulty Geertz (1988: 1) saw in ethnography - "how to sound like a pilgrim and a cartographer at the same time" - requires resolution. Treatment of a disciplined account of discipline as an ethnographic 'fiction' (cf. Clifford 1986; Latour 1988a; Knorr-Cetina 1994), where this implies an embrace of epistemological limits, is one means of doing so. It allows the subdisciplinary to be advanced as the disciplinary, the specific as the general, the pilgrimage as the mapping.

As a 'politics of representation' (Lee and Ackerman 1994), ethnographic fiction entails that re-fusion of the epistemological and the political found in Mannheim's work and in actor-network theory. To be sure, postmodern revisionists have held that through the privileging of the writer over both readers and represented subjects ethnography generates a repressive alterity (Clifford 1986; Marcus 1994), a concern also in the sociologies of scientific knowledge (e.g. Mulkay 1985; Woolgar 1988b). Yet to worry about 'otherness' is to beg the question of how 'sameness' is constituted. As Latour (1988a: 168) has noted, "[r]eaders seem to be much more devious, much harder to take in, much cleverer at deconstruction, much faster in fiction-making" than such concerns would suggest. The

¹⁶ Callon (1986) stressed the overlap of the two senses of 'translation': *traduttore-traditore*.

fact of represented difference is then of less interest than are the uses of representations, and as in the stress on the audience in the 'new rhetoric' or in the actor-network theoretical claim that the fate of texts is decided by readers, that is not a matter of meta-generic *diktat*. Limited by what its readers will accept, the fiction deriving from textual ethnography allows a non-legislative reconciliation of the sub- and metadisciplinary.

Collective like society: Textual ethnography: Reprise

The third element in Mannheim's, Kuhn's, and the actor-network theorists' triads, subsuming any number of exemplars, is adapted here to the contention that quantitative analyses and rhetorical studies of knowledge entail the collectively lived circularities of ethnographic reading and writing. The lived was linked to the quantitative, for example, in Altheide's (1987) description of one study as 'ethnographic content analysis', and to the rhetorical in Clifford's (1988) stress on the synecdochic quality of participant-observation. 'Textual ethnography' was intended to allow for reflexivity, and once such embodied prejudice is accepted as inevitable, self-applicability follows. So does the same jagged regularity as found in quantitative and rhetorical methods. The locations defining a textual ethnographer set one range of contingencies, and the collectivity enabling textual ethnography another. Pervading subdisciplinary use of the other two sets of methods, this third also shares their metadisciplinary ordering .

At that point, the distinction between the two forms of the disciplinary ceases to be useful. Just as the local is at all points global, and vice versa, the subdisciplinary is at all points the metadisciplinary. 'Fractal ordering' describes that simultaneity.

FRACTAL ORDERING

A recurrent pattern has been noted in the three 'modes of ordering' surveyed in this chapter: the meanings derivable from quantitative analyses, rhetorical study, and ethnographic reading/writing are all systematic in dispersion, or non-arbitrarily contingent. Repeating generally the orderings of the sociologies of knowledge and of

'discipline', and reappearing in detail in the study of 'age', it is simultaneously sub- and metadisciplinary. That trans-scalar recurrence can be summed up in a Kuhnian 'preferred analogy' for the approach and findings in this first part of the thesis: a sociology of disciplinary knowledge is a 'fractal ordering'.¹⁷ Since the 'chaos' from which the image is taken denotes the apparently random behaviour arising from the sensitivity to initial conditions of a deterministic system, 'fractal ordering' is intended to suggest the regularity in dispersion, the repetition across scale, and the reflexivity found in this chapter and in the preceding two. After a note on 'fractals' and 'strange attractors', the analogy will be used to summarise the study of disciplinary knowledge.

Fractals and strange attractors

Two features of fractals are metaphorically appealing, the reconciliation of order and disorder, and self-similarity over scale. In a much quoted description, Mandelbrot (1983: 1) located his coinage of the term in a contrast between the 'fractal geometry of nature' and Euclidian order: "[c]louds are not spheres, mountains are not cones, coastlines are not circles, and bark is not smooth, nor does lightning travel in a straight line". Where earlier mathematicians had treated as a 'gallery of monsters' curves which did not obey the integral dimensions of Euclidian space, he outlined a geometry of fractal dimensions to model the observed jaggedness of nature; coastlines, for example, were found to have dimensions falling between the one and two of the line and the plane. As in the paradigmatic 'Mandelbrot set', the curves in this geometry were self-similar over scale. This replication of their ordering at any magnification allowed regularity within apparent irregularity to be modelled in minute detail. That same sense of ordered disorder is implied in one form of fractal, the 'strange attractor'.

Where Mandelbrot opened the simplifications of Euclidean space, others had questioned the states of equilibrium attributed to dynamic systems through the use of linear equations as approximations. In non-chaotic systems three states of equilibrium are possible - point attractors, limit cycles and tori - but when a system is sensitive to its initial conditions, a fourth possibility emerges: a system is said to have a 'strange

¹⁷ cf. Barnes' (1974: 166): "We may say that the sociologist metaphorically redescribes theoretical accounts as metaphorical redescription".

attractor' when its long-term behaviour is apparently random but still organised. While all solutions to the equations fixing the system fall within the attractor, any two arbitrarily close starting points lead to widely divergent outcomes. The strange attractor's equilibrium is then both determined and contingent.¹⁸

Quantitative study of chaos has met only limited success in the social sciences (Brown 1995), but its metaphors have been widely adopted. Their use here is broadly consistent with, say, Reisch's (1991) reconciliation of narrative and the nomothetic, Baker's (1993) chaotic reading of the problem of order, or Newman's (1996) treatment of emergent events.¹⁹ More specifically, it has the transhistorical continuity of Serres' reading of Lucretius on turbulence, of his treatment of history as chaotic, and of his disruptions of linear time (e.g. Serres and Latour 1990; Serres 1977). These themes appear in depiction of the study of disciplinary knowledge as 'fractal ordering'.

Fractal ordering

The study of disciplinary knowledge must accommodate the problematics of self-exemplification and relativism epitomised in 'Mannheim's paradox'. As an extension of Law's (1994: 83) 'modes of ordering' - "fairly regular patterns that may be usefully imputed for certain purposes to the recursive networks of the social" - and as a description of the 'fairly regular pattern' found in the sociologies of knowledge, in studies of 'discipline', and in the methods used in both, 'fractal ordering' is addressed to those problematics. It allows a metaphorical summation of the self-similarity found across different levels of reading, of the recurrence of regular irregularity, and of the 'modesty' implied by self-exemplification.

Mathematicians' avoidance of the non-Euclidean 'gallery of monsters' recalls the purifying denial of teratology in attempts to avoid 'Mannheim's paradox'. As so often Merton both presaged a solution and epitomised purification. In his paper on 'unanticipated consequences', written contemporaneously with his thesis, he quoted a

¹⁸ This reading is taken from popular accounts: Gleick (1987); Stewart (1989); Ruelle (1991).

¹⁹ Young's (1991) description of chaos as a 'metaphysics of the postmodern', derived from a privileging of the contingent over the determined, is another matter. So too is Kellert's (1993) epistemological skirting of the sociology of knowledge.

point made by Poincare - "small differences in the initial conditions produce very great ones in the final phenomena" (in Merton 1936b: 899) - widely taken as anticipating chaos theory.²⁰ But this was among the elements he never reconciled with his calls for lawful scientificity in sociology. While mathematical laws might be out of reach, the concurrent fracturing and orderliness of fractals does describe the patternings derived from the lifting of his foreclosure on epistemic monsters. That was the regular irregularity found in the rhetorical moves in the sociologies of knowledge, in the real, discursive and collective moments in the study of 'discipline', and in the quantitative, intertextual and ethnographic structuring of disciplinary findings.

Self-similarity across scale, given in that repetition and implied by 'fractal', is also a familiar theme. It appears, for example, in Durkheim's (1902: 243) claim that "there is in every civilisation a kind of tonality *sui generis* which is to be found in all the details of collective life", or in the romantic-conservative moment of the classical sociology of knowledge. Blake's dream of seeing the world in a grain of sand, echoed in Mannheim's (1921-2) claim that his 'documentary method' allowed extrapolation from the smallest detail in a work of art to a total *Weltanschauung*, is re-echoed in any attempt to take knowledge as symptomatic.²¹ Or again, it is given in Clifford's (1988) treatment of participation observation as synecdochic. It has recurred in that sense in the textual ethnography of this and the previous two chapters. The triadic interaction of real, discursive and collective moments was repeated not only in the sociologies of knowledge, in studies of 'discipline' and in disciplinary methods, but in details within each as well. Mannheim's layered trinitarianism, Foucault's trinary orderings, or the repetition of the overall patterning in feminist studies and in treatments of age, period and cohort effects, all suggest the trans-scalar generality given in the image of fractal.

Finally, it is obvious that an emergent 'fractal ordering' informed the reading of the sociologies of knowledge, the modelling of the disciplinary, and the discussion of disciplinary methods. Once trinitarian avoidance of purification was derived from the

²⁰ Gleick (1987: 321), Stewart (1989: 298), and Ruelle (1991: 48) all quoted that passage.

²¹ That is an ancient imaginary. Polybius (1979: 439), for example, mentioned "a proverb which tells us that a single drop, taken from even the largest vessel, is enough to reveal the nature of the whole contents".

homology between Mannheim's, Kuhn's, and the actor-network theorists' work, it was adopted as an architectonic of ordering. The textual ethnography of the three chapters of Part I entails a Pascalian seeking for the contingently determined which had already been found, or a hermeneutic spiralling between reading and read, writing and written. Since 'fractal ordering' is then itself a fractal ordering it accommodates the self-exemplification and relativisation required of any sociology of knowledge. It is a means of refusing to treat 'Mannheim's paradox' as a paradox.

That reconciliation of the contingent and the determined in 'fractal ordering', that general self-similarity over scale, and that specific self-exemplification, allow reformulation of the simultaneously sub- and metadisciplinary character of the study of disciplinary knowledge. Rather than being distinct, the two moments are mutually fractal. Study on a small scale leads to the general, but the globalising general is always undercut by the specific. On that basis, the argument of Part I of the thesis - that sociologies of knowledge are representations of discipline - can now be summarised.

SOCIOLOGIES OF KNOWLEDGE AS REPRESENTATIONS OF DISCIPLINE: REPRISE

Dual objectives framed this first part of the thesis: to develop a formal sociology of disciplinary knowledge; and to derive from that a standard and a means for comparison with informal accounts. The first was directed to the puzzle of sociology's location in what is studied within it. 'Discipline' was taken as paradigmatic of that process, and the sociology of knowledge as paradigmatic of sociology in general; debates over the self-dissolution imputed to 'Mannheim's paradox' are almost definitive of the field, and the problematics in it of self-exemplification and relativism are also those of sociology at large. The reconciliation of order and disorder under 'fractal ordering', as a summary of the Mannheimian, Kuhnian and actor-network theoretical readings of the sociologies of knowledge, of studies of 'discipline', and of disciplinary methods, was intended to accommodate the uncertainty and circularity to which they give rise. Fulfilment of the two objectives of Part I is now summarised in those terms. Sociologies of knowledge are

shown as representations of discipline, and the means for informal comparison derived from them.

Formal sociologies of knowledge

The starting-points for the reading of the formal sociologies of knowledge were Law's (1994) observation that ordering strains towards dualism under modernity were a sociological topic, and Latour's (1991) depiction of a particular form of dualism. Once nature-culture was treated as an emergent effect of networks, the modernity of sociology's self-location could be defined by the 'work of purification' in denial of the hybrid 'work of translation' by which it was achieved, and 'purification' in turn by a separation of the political and epistemological senses of representation. Sociologies of knowledge are paradigmatic in that sense, for the purification evident in variations on the distinction of civilisation from culture rested first, on claims to epistemological warrants beyond politics, and then, on claims to political authority on that basis. As in Wirth's (1936: xxv) contention that sociologies of knowledge are especially salient in "epoch[s] marked by profound social and intellectual upheaval", and in identification of a revival of interest in them with the condition of late modernity (e.g. Frisby 1992; Kettler and Meja 1995; McCarthy 1996), they have been widely taken as symptomatic. 'Discipline' is just as paradigmatic, in the same way, and in both its use as an overall descriptor and methods for the study of its effects; the pun on rationalisation given in it requires the same rupture between the epistemological and the political, followed by the same asymmetrical re-fusion.

So a reading of 'discipline' through the sociologies of knowledge is a prime means of access to sociology's location in what is studied within it; trope and tradition are both representative. Such a reading was undertaken by a reconciliation of rhetoric with Law's 'orderings' and Latour's 'networks'; tradition and tropes both imply the co-emergence of epistemological and political authority in achieved representation. As seen in recurrent tensions in the sociologies of knowledge, in accounts of discipline, and in methods for the study of disciplinary knowledge, and as epitomised in trinitarian 'fractal ordering', that reading yielded a resolution of 'Mannheim's paradox'. Since the

diagnosis of 'paradox' is open to the *tu quoque* argument (Ashmore 1989), it is not amenable to strictly rational solution. Once the givenness of rationality is at issue, the purification in the sociologies of knowledge is self-tainting, and the performative pun on discipline is catachrestic. Instead, a means of riding the circularities is required, a means, that is, of self-exemplifying the achievement of rationality and the contingency of representation.

Trinitarian fractal ordering is such a means. As the next simplest alternative to dualism, ordering by threes recurred in acceptances of teratology in the sociologies of knowledge. It was used to pattern the disciplinary into disciplinarity, disciplining and discipline, to break disciplinarity into optimistic imaginaries of collectivity, science, and usefulness, and to take discipline as a failing effect of documents, devices and drilled bodies. It was used again to treat disciplinary knowledge as at once real like nature, narrated like discourse and collective like society, and to treat disciplinary methods as scientific/quantitative, rhetorical/ discursive, and ethnographic/ collective. That trans-scalar isomorphism, or fractal ordering of disciplinary rhetorics, is then a representation of the sociologies of knowledge as representations of discipline.

That formal reading of the sociologies of knowledge sets the standard and means for comparison with informal versions of them.

Towards a folk sociology of disciplinary knowledge

Any set of texts which is directed to disciplinary knowledge, which is not couched in terms of the formal sociologies of knowledge, but which is amenable to the fractal ordering of the disciplinary, amounts to an informal sociology of knowledge. Such a folk sociology could be developed from any expression of the interaction of the documents, devices and drilled bodies of a discipline: introductory textbooks; referees' reports for disciplinary journals; the structure of doctoral programmes; applications to external funding agencies; the organisation of conferences; codes of ethics; book proposals. Any aspect of a discipline would serve. But whichever is studied, the disciplining moves within it should be expected to entail optimistic appeals to the collectivity, science, and usefulness of disciplinarity, and should be open to the fusion of quantitative, discursive

and ethnographic methods. In whatever context, 'real' and 'ideal' factors are open to limited use. In whatever context, collectively infused repertoires of science and usefulness should be expected. In whatever context, the discipline is lived, and lived collectively. Comparison of formal and informal through fractal ordering then does not require a dualistic separation; rather, it is a matter of contextually different resolutions of the same rhetorical possibilities.

Now, just as the sociologies of knowledge and discipline epitomise sociology's location in what is studied within it, a folk sociology is available which epitomises the relation between formal and informal: presidential addresses to the American Sociological Society/Association. Where the sociologies of knowledge were shown throughout this first part of the thesis as representations of discipline, the presidents of the ASS/A, representatives of the discipline, can be shown as folk sociologists of knowledge; exemplifying the rhetorical opening of the foreclosures characteristic of the field, formal and informal are then antimetabolic rather than antithetic.

After the argument of Part I has been exemplified in a case-study of Lazarsfeld's {1962} address, that case will be developed in Parts II and III

CASE 4 : LAZARSFELD'S METHODOLOGY

Lazarsfeld's {1962} discussion of 'the sociology of empirical social research' is apposite at this point in the thesis. It illustrates the argument in Part I, it foreshadows that in Parts II and III, and so it exemplifies the antimetabolic relation between formal and folk sociologies of sociological knowledge. That dual potential is given in Lazarsfeld's standing as a methodologist and in his having reflected on his address' situatedness, more than did any other president but Goffman (1982). To resolve his occasioned nervousness over a topic - '[m]ore irrevocable than marriage, more self-revealing than a dream' - he looked to the early presidents of the ASS. Elected for two-year terms, they had delivered two addresses:

One was usually devoted to a specific sociological problem that concerned them, the other to a kind of state of the union message, in which they discussed matters currently of concern to our profession at large.

Since he merged those sub- and metadisciplinary moments, and since his defence of 'methodology' is among the few presidential approximations to a formal sociology of knowledge, his address epitomises the way that sociologists of knowledge represent discipline, and that, representing the discipline, the presidents are sociologists of knowledge.

This note on his address is structured around those antimetabolic poles. First, Lazarsfeld's 'methodology', as presented in the address and elsewhere, is shown to be consistent with the sociology developed in Part I, and then his address, as address, is read to illustrate the folk version of it.

When Lazarsfeld used his address to 'trace back some of the things you know I stand for', he referred to methodology and the organisation of empirical research. His account of their interrelation amounts to an actor-network theoretical ordering: if "[t]he sociologist studies man in society [while] the methodologist studies the sociologist at work" (Lazarsfeld 1959a: 40), then his work is a sociology of sociological knowledge; if there is "an intrinsic relation between research organization and methodology" (Lazarsfeld 1969: 291), then knowledge is material; if "manageable knowledge" is a disciplinary aim (Lazarsfeld 1959a: 40), then a joint stress on methods and organisation entails a resolution of the epistemological and political senses of 'discipline'.

Methodology, Lazarsfeld (1959a: 78) held, typically and undeservedly evoked

a “mixture of contempt and anxiety” among sociologists.¹ On his account, methodology was neither the set of prescriptions nor the technology usually ascribed to it. Being less rigorous than formal logic, it was not philosophy, and, having less substantive content, it was not sociology of knowledge (Lazarsfeld 1959b: 239). It consisted, rather, of the “positive critique of scientific language” (Boudon 1971: 81). Lazarsfeld noted several elements in its genealogy. ‘Critique’ was used in both Kantian and Marxist senses, as analysis of the conditions under which sociological thinking was possible, and so as an uncovering of foundational assumptions (Lazarsfeld 1959b: 237-8). Since that entailed a search for structure in researchers’ language, methodology was a form of *explication de texte*, or semantic analysis (Lazarsfeld 1959b: 237). Sometimes allowing the reasoning beneath researchers’ practices to be mathematised, it bore “an obvious similarity” to the positivism of the *Wiener Kreis* (Lazarsfeld 1969: 273), but, having roots in Dilthey’s hermeneutics, it was also a mode of self-understanding. For Lazarsfeld:

methodology was a means of disciplining his passion for clarity and integrity of thought. It was a matter of discovering what one truly wanted to say, of explicating the grounds that allowed it to be said responsibly and, not least, of indicating the sort of evidence that would lead one to confess to having been mistaken (Merton 1980: 20).

The resemblance to the sociologies of knowledge even extends to versions of the trialism recurrent in them: genealogically, in positivism, textual analysis, and hermeneutics; or in the sources of knowledge Lazarsfeld (1969: 280) claimed to have learned as a student and to have retained throughout his work - observation of behaviour, interpretation of cultural products, and introspection. Methodology is then a fractal of the sociologies of knowledge.

Lazarsfeld had one of the prime qualifications for what is “traditionally the concern of marginal men” (Remmling 1973: 7), for that was how he presented himself. Sills (1979: 422) reported an oral history in which he ascribed his marginality to his Jewishness, his foreignness, and his interest in market research. Arriving in America with a background in humanistic left politics, mathematics, and psychology, he was slow to identify himself with sociology.² But this distance was also an advantage, for as he told Stehr (1982: 150), empirical research in sociology was so little developed at the time of his arrival that “[w]hat was needed most were amateurs who knew a little bit in a number of fields, and I fitted this picture very well”. His institution-building,

¹ Mills (1959), of course, attacked his ‘abstract empiricism’.

² Merton (1980: 20) described him as “an amiable but perplexed traveller in the exotic land known as sociology”.

too, was a typical product of “the marginal man who is part of two different cultures” (Lazarsfeld 1969: 302).

Especially in the BASR at Columbia, Lazarsfeld pioneered the organising of empirical work in ‘research institutes’. The director of an institute was a ‘managerial scholar’ or ‘idea broker’, whose administrative work was necessarily methodological. Practical decisions in the coordination of experts - in interviewing, statistics, literature reviews, and the like - induced a reflexivity which ‘quickly turns into methodological explication’. While Lazarsfeld saw this ‘intrinsic relation’ between organisation and methodology’ in neo-marxist terms, as expressing a link between productive technology and analytical means, it could also be taken as exemplifying actor-network theoretical heterogeneous engineering. Like all problematic translation chains, his institutes were articulated to social order more generally.

At the time of his address, around sixty such institutes had been established in American universities, but their place in the academic structure was still controversial. Lazarsfeld held that the uneasy relation between scholarship and administration had resulted in a ‘power vacuum’ in universities which ‘managerial scholars’ were well placed to fill. Since no one best way could be prescribed for them, their practices would be pluralistic, but ‘pluralism is not the same as anarchy, and it is anarchy with which we are faced at the moment’. This problem of order required that methodological rigour be matched by organisation, and while Lazarsfeld barely used the word ‘discipline’ in his address, he linked those two forms of rationalisation in granting that he was seen as having brought a ‘bureaucratic disenchantment to the world of scholarship’. But his own interfusion of research and organisation appears to have been less Taylorite than Machiavellian, in the actor-network theorists’ sense.³ His use of the history of sociology is paradigmatic.

Given the mistrust of applied research, its “legitimation, like women’s work, seems never to be done” (Lazarsfeld 1969: 283). One means that Lazarsfeld recurrently used was a rereading of the history of sociology. At his instigation, for example, Zeisel wrote a brief history of ‘sociography’ to be included with the early Marienthal study, “in order to extend the intellectual pedigree of our study” (Zeisel, in Oberschall 1978: 200). Lazarsfeld repeated the technique when he located Le Play and Quetelet in the

³ Callon, Latour and Law refer frequently to Machiavelli’s allowance for contingency, to his stress on potentially treacherous alliances, and to the emergent senses of power and morality in his work (e.g. Callon and Latour 1981; Latour 1988b; Law 1994). Coleman (1980) used Don Juan rather than Machiavelli to describe Lazarsfeld’s manoeuvres.

sociological mainstream,⁴ read Durkheim as a pioneer of multivariate analysis, or uncovered an 'other' Weber who had turned to quantitative analysis (Lazarsfeld 1961; 1959a: 55-6; Lazarsfeld and Oberschall 1965). In this he took at least the possibility of definitive history for granted, using Orwell's 1984 to warn against rewriting the past (Lazarsfeld 1950: 75), and looking to resolution of the puzzles he was to retrace in his address, such as why empirical social research did not become institutionalised in Europe when its roots could be found there. But he also saw history as written from particular positions for particular purposes, thus shifting the puzzles from the past to the present; in this sense he took advantage of the imprecise boundaries of sociology (e.g. Lazarsfeld 1961: 97) to stretch its history to his own needs:

the conventional university departments were never suited to the execution of research projects ... Most universities created centers, laboratories, bureaus, organizations which were not well integrated into the academic departments, were badly financed, and yet were indispensable for the training of students. The battle for these and around these hybrid centers and their financial support is still raging. My interest in the history of empirical social research is part of this battle. By showing that empirical social research has a dignified intellectual pedigree of its own, one could strengthen the claim that it should have a better academic representation (quoted in Oberschall 1978: 203).

In the recognition of 'hybrids', in the depiction of knowledge as agonistically emergent from them, and thus in the implicit fusion of the epistemological and political senses of 'representation', this is proto-actor-network theory.

Given his long association with Merton, when Lazarsfeld distinguished his methodology from the sociology of knowledge he no doubt referred to Merton's reading of it.⁵ But his treatment of sociological knowledge as an effect of documents, devices (organisational forms), and drilled bodies, and his tacit collapsing of Merton's binaries, prefigure more teratological accounts of the disciplinary. The assemblage of his versions of the trinary structuring of knowledge and its study, of the manageable knowledge given in the relation between methodology and organisation, and of his legitimacy imagined tradition, suggests the heterogeneous engineering of actor-network theory far more than it does Mertonian purification.

Since Lazarsfeld's work is then consistent with the formal account developed in Part I of the thesis, it allows a contrast with the folk variant in his address.

⁴ When Columbia honoured him with a named chair, he became the Quetelet Professor of Social Science.

⁵ A student and colleague of both, Boudon (1993: 22) similarly held that Lazarsfeld's address should not be read as a contribution to the sociology of sociology.

It will be argued in Parts II and III that occasioned constraints on presidential addresses partly reinforce and partly conflict with their occasioning as sociologies of disciplinary knowledge. This was seen in the other case-studies. A blunted reflexivity marked Goffman's {1982} suggestion that addresses entail disciplinary honour, disciplinary policing, and disciplinary exemplification, Wirth's {1947} and Gamson's {1994} evocations of collectivity, and Gans' {1988} illustration of a match between the addresses and the modelling of disciplinarity, disciplining, and discipline. Through the recurrence of that effect in Lazarsfeld's, it is now suggested that the collectivity common to the three moments of generic distinctiveness and the three moments of the disciplinary imposes constraints which are themselves situatedly distinct.

Perfectly cast as an after-dinner talk, Lazarsfeld's address shows the fusion of disciplinary honour, policing, and exemplification of its occasioning, and thus enacted the moral affirmation attributed to ceremonial speech in both rhetorical and discourse analyses (e.g. Perelman 1977; Mulkay 1984). Since Lazarsfeld took as one point of departure the 'unfair misunderstanding of methodology',⁶ and the controversies over institutes as another, since he treated the 'dangerous divergence' between intellectual and administrative work, and since he held their reconciliation as a desideratum, his address was simultaneously a response to personal recognition, a boundary-riding of the discipline, and an exemplification of a possible future. Since he managed all that with a wryly disarming humour, since he enrolled such disciplinary notables as Durkheim, Toennies, Weber, and Parsons, since he included Small, and Chicago sociologists in general, who might be taken as representing a rival position, and since he reassigned credit to such patrons and friends as Buehler, Lynd, Stouffer and Merton, his meeting of generic requirements amounted to reaffirmation of an embrative collectivity. But that then limited the reflexivity of the work for which he was being honoured.

Both of Lazarsfeld's interwoven themes, methodology and organisation, were given as reflexive, and his own layering of that was noted earlier. But while the general consonance of his work with the sociologies of knowledge recurred in details of the address, the effect was muted. Any sociology of knowledge was shown to entail representations of discipline, as perennially failing, of disciplinarity, as an eternally optimistic imaginary of science, usefulness, and collectivity, and of disciplining as a linking of them. All these appear in the address. The failing of 'discipline' is found in

⁶ '[T]hose who can, do; those who cannot, teach; and those who have nothing to teach, become methodologists'.

Lazarsfeld's 'dangerous divergence', or in a perception of disciplinary fragmentation:

until 1937 our annual meetings always had a central theme; then a resolution was adopted that because the field had become too diversified, this practice was no longer possible. By now it would seem that diversification has reached such a point that the annual meeting should perhaps try to review common denominators, one by one.

The collectivity of 'disciplinarity' is occasioned, while its science is given in the rigour Lazarsfeld imputed to methodology, and its usefulness in his invocation of 'the feeble authority of the President' not only to restore a unifying theme for the annual meeting but also to persuade the Council that 'the problem of utilization is an urgent one'. That is sharp enough. But the 'disciplining' reconciliation of incomplete 'discipline' with the optimism of 'disciplinarity' does not match it; Lazarsfeld's objection to the 'unfair' reaction to methodology, for example, could hardly be called vigorous policing. This moderation is all the more striking, given that his substructural sense of methodology was perfectly suited to examination of collective taken-for-grantedness.

Here is the difference between formal and folk disciplinary rhetorics. Each entails a version of disciplinarity, disciplining and discipline, but the collectivity affirmed situationally in fusions of disciplinary honour, policing and exemplification is quarantined situationally from the reflexive scrutiny of formal study. To be more than tangentially reflexive would be to breach collectivity. As a quasi-sociologist of knowledge, then, Lazarsfeld wrote formal representations of discipline, but as a representative of the discipline, he sketched a folk sociology of knowledge.

The background for elaboration of that argument is presented in Part II, on 'the discipline' of American sociology.

PART II:

‘THE DISCIPLINE’

‘Discipline’ epitomises sociology’s location in the processes studied within it, and the sociology of disciplinary knowledge of its representation is a fractal ordering; that was shown in Part I. Now, presidential addresses to the American Sociological Society / Association will be shown to amount to a folk sociology of disciplinary knowledge. As foreshadowed in the case-studies, these artefacts of “what we still call *the discipline*” (Gans 1990: 446) are amenable to fractal ordering, and it remains now to ground them more securely in their American context.

‘America’ is as paradigmatic as ‘discipline’ of contemporary processes, and American sociology then doubly so.¹ Shils’ (1960: 78) claim that “for better or for worse it is sociology, *tout court*, over the world today” requires softening, but is still salient.² American books and journals are international staples, the traditions imagined in them are international points of reference, and ‘the discipline’ is implicated in the processes lived and studied internationally.³ American sociology is a colonising effect. This is what allows an Australian who has never visited America, let alone been directly disciplined in American sociology, to study the disciplinary through it.

Local sociology is heterogeneously engineered as globally American: in the American disciplining of local sociologists; in versions of American organisational forms; in the adoption of American reference groups; and above all in the American texts which are disciplinary correlates of the American fiction, American cinema, and American music mediating any life under modernity. Evoking the same sense of the familiar and the exotic, American sociology is amenable to a textual ethnography. To

¹ “By 1900, a person could devote himself fully to being a sociologist by employment as a salaried teacher of sociology in the United States. In no other country of the world, except in France, did this possibility exist and in France it was realised only for Emile Durkheim, while in the United States several hundred persons could do so” (Morgan 1982: 53).

² cf. Ferree and Hall’s (1996: 930) ‘mainstream sociology’, which “excludes forms of sociology practiced outside the United States”.

³ Touraine (1990: 252), for example, held that “American sociology is one of the intellectual creations that has most deeply influenced our century”, and Connell (1990: 270) that “[s]ociology is not only conditioned by American power. It is itself an aspect of American power”. Or again, “[i]n most countries that could afford to have sociology at all, the discipline was created or remade in the 1950s and 1960s on the basis of research techniques, research problems, and theoretical languages, not to mention textbooks and instructors, imported from the United States” (Connell 1997: 1544).

read it is to circle between jolts of strangeness and the comfortably known. On that basis, this second part of the thesis consists of two chapters, drawn from the other two methodological moments of fractal ordering: 'the discipline' will be treated in Chapter 5 as 'narrated like discourse', and in Chapter 6 as 'real like nature'. Each chapter is intended as both an advancing of the argument in its own right and a setting of the context for the rhetorical reading of presidential addresses in Part III. Drawn from accounts of American sociology, of the presidency of the ASS/A, and of presidential addresses, Chapter 5 will be used to show the generic distinctiveness of the addresses, its fit with fractal ordering, and thus the collective standing of the addresses as a folk sociology of disciplinary knowledge. In Chapter 6, the 'real factors' of the presidents' organisational characteristics will be related quantitatively to 'ideal factors' in their addresses. Since results are again consistent with fractal ordering, the sociology of disciplinary knowledge from Part I is supported. At the same time, their jaggedness warrants the reading for disciplinary rhetorics in Part III.

CHAPTER 5 : AMERICAN SOCIOLOGY AND PRESIDENTIAL ADDRESSES

Presidential addresses to the ASS/A will be located in this chapter within accounts of American sociology in general. It must be stressed that the unity suggested by 'American sociology' is potentially misleading; the aims, methods and concerns of 'the discipline' have been disputed from its proto-formalisation in the mid-nineteenth century. But as Wolff (1946: 545) said in his call for a 'socio-cultural interpretation' of it, "[i]n the present context ... any referent of 'American sociology' which the reader may have in mind will suffice", and since the referent here is not a presumed unity but its fractal achievement, the descriptor can stand. An ordering of accounts on the assumption that disciplinary networks are 'narrated like discourse' does not imply a preemptive fixity.

After a sketch of the formalisation of American sociology, the reading will be developed through three narrowing foci: 'the discipline' will be fitted to the modelling of 'discipline' as perennially failing, the presidency of the ASS/A will be shown as an uncertain effect of it, and presidential addresses will in turn be seen as situated effects of the presidency. As well as setting the terms for the quantitative analysis in the next chapter and for the rhetorical study in Part III, the chapter amounts to a test of fractal ordering against the Parsonian-Mertonian common sense of 'the discipline' and of a Mertonian-Kuhnian common sense in accounts of it. The diagnoses of 'crisis' in structural functionalism which allow 'the discipline' to be matched with 'discipline' entail Mertonian-Kuhnian assumptions, since 'crisis' appears as a purifying response to the interfusion of the epistemological and the political, rather than in its paradigmatic sense. The presidency is part of a Mertonian 'reward system' of sociology, but is more contingently related to disciplinary knowledge than Merton's model can accommodate. Since presidential addresses will be shown as enacting 'Mannheim's paradox', they are more amenable to repertoiral/rhetorical study than to Mertonian analysis; the representations of disciplinary knowledge in them are effects of the presidents' uncertain representativeness and of the occasioned constraints following from it. That, in turn, allows the disciplinary rhetorics in the addresses to be read as a folk sociology of disciplinary knowledge.

FORMALISATION OF 'THE DISCIPLINE'

Within the documents, devices and drilled bodies of a discipline, organisation is a device, and an account of American sociology then requires a sketch of its formalisation. Amid a tradition of disciplinary reflexivity, that outline at least is agreed. It also has a *prima facie* fit with the argument in Part I. If it is "an historical cliché that the sociologists responsible for the initial phases of the discipline's institutionalization were in large part motivated by an interest in social reform" (Kuklick 1980: 824),¹ then the process was always already politically epistemological, or a resolution of science, usefulness, and collectivity. It is shown here as entailing reconciliations of centripetal and centrifugal strains.²

When the American Social Science Association was founded in 1865 it was a crystallisation of religious, political and intellectual interest in a 'science of society'. Identified by the formation of national associations, the familiar disciplines were differentiated from within it. As suggested by the timing, the domain claimed for organised sociology was residual in character, even if some early figures held that it was a synthesis of all social knowledge and so was the basic social science (Haskell 1977: 190).³ The first course in it may have been taught by Sumner at Yale in the 1870s, the first department was established at Chicago in the 1890s under Small, and the first national journal, the *American Journal of Sociology*, was founded there soon afterwards. When the American Sociological Society was formed in 1905, with Ward as the first

¹ The founders' Baconian/Comtean imaginary was clear. Kuklick noted Lester Ward's 'sociocracy'; Friedrichs (1970: 72) quoted Ward's claim that "the true guide, the Moses that is to lead man out of the wilderness, is science". Albion Small's (e.g. 1896: 564) version was all but explicitly Comtean: "I would have American scholars, especially in the social sciences, declare their independence of do-nothing traditions. ... I would have them advance from knowledge of facts to knowledge of forces, and from knowledge of forces to control of forces in the interest of more complete social and personal life".

² The extent of apologetic, critical and historical writing on American sociology is in itself indicative of this tension. Information here is taken from published records of the ASSA and from texts cited in this and other chapters.

³ This chronology of disciplinary formations is adapted from Geiger (1986: 23-4):

1865	American Social Science Association
1883	Modern Language Association
1884	American Historical Association
1885	American Economic Association
1889	American Academy of Political and Social Sciences
1892	American Psychological Association
1901	American Philosophical Association
1902	American Anthropological Association
1903	American Political Science Association
1904	Association of American Geographers
1905	American Sociological Society

president, a degree of formal unity was achieved despite continuing division over religious, political and scientific imaginaries of 'the discipline'.

The Society's executive comprised past presidents, the president, a first and second vice-president, and six members of council, with two of these elected each year and each serving three-year terms. The first five presidents were elected for two-year terms; incumbency thereafter was one year. Candidates for office were nominated by a committee appointed by the president, with provision for nomination by the members at large. In early effects of simultaneously centrifugal and centripetal dynamics, the first regional sociological societies and the first specialised sections within the ASS itself were formed in the early 1920. Tensions were also evident on a larger scale, for Chicago was privileged within national organisation: the Society's secretary/treasurer was drawn from the department there, and the *AJS* was its official journal. In a 'rebellion' at the meeting of 1935 (Lengermann 1979), it was decided that the Society's base be removed from Chicago and that the *American Sociological Review* be founded as its official journal.

In minor reforms over the next fifteen years, the dispersion of the membership was acknowledged through the introduction of postal voting to replace balloting at the Annual Meeting. More centripetally, a position of part-time executive officer was created. In another major constitutional revision, taking effect in 1951, the council was enlarged to twelve, with delegates included from the regional and special-interest societies, and with an executive committee elected from within it. Minor reforms of this constitution included creation of the office of president-elect, and replacement of the second-vice-presidency by a vice-president-elect. More significantly, the Society became a professional Association,⁴ with an executive officer and a secretariat based in Washington DC; Turner and Turner (1990: 152) saw the change of name as connoting "a broader base of representation and a less intimate and cohesive form of organization".

Another constitutional overhaul was enacted in 1967. In a centralising move, representation from the regional and special-interest societies was abolished, but the system of nomination of candidates was broadened to guarantee regional inclusiveness, the provisions for write-in candidates were eased, the secretaryship became an elective office, and formation of sections became more simple. Here again was a balancing of centripetal and centrifugal dynamics. Increasing specialisation of sociologists suggests

⁴ This was the occasioning of Parsons' (1959) paper on sociology as a profession.

that centrifugality has been transferred to within the Association; the five organised sections in 1960 had become twenty-five in 1987, and thirty-seven in 1997. Conversely, moves to improve the access of women and members of minority groups have continued the simultaneously centripetal and democratising inclusiveness characteristic of the Society/Association since its formation.

Its membership has fluctuated: after increasing from 1905 it declined during the Depression, rose sharply after World War II until a peak in the early 1970s, declined again until the mid-1980s, and has risen slowly since then. The theoretical schools and movements found in textbooks have similarly waxed and waned. While organisational and theoretical flourishing coincided in the 'golden age' of the 1950s and early 1960s (Rhoades 1981), when increasing membership matched a degree of consensus around Parsonian-Mertonian sociology, disciplinary stability was always provisional.

Alternatively, 'provisional stability' might be seen as 'permanent crisis'.

'THE DISCIPLINE' IN CRISIS ?

The constitutional revision of the 1960s which marked the ending of the 'golden age' in American sociology coincided with the more general upheavals of the times. Following a demonstration at the annual meeting of 1967, for example, against both the war in Vietnam and the conduct of the ASA, the Radical Sociology Movement was more or less formalised, and had an impact at the next two meetings. After a perceived lack of response to a demand for an expanded role for black sociologists in the workings of the ASA, the Black Caucus moved in 1969 to split from it. The Women's Caucus also met for the first time at the meeting of 1969, calling for greater equality within the discipline (Gamson 1968; Roach 1969; Rossi 1970). This disciplinary legitimisation crisis entailed widespread disciplinary scrutiny. But if the end of the 'golden age' was the end of the Parsonian-Mertonian consensus, the break was not clear. *The American Sociologist*, the vehicle of much of this disciplinary examination, had been founded in 1965 under Parsons' editorship, and much of it was conducted in Mertonian-Kuhnian terms. As seen in the sociology of sociology which flourished from the late 1960s, and in the critiques of the discipline which have continued to the present, this disciplinary scrutiny entailed a recurrence of familiar difficulties; 'the discipline' matches the modelling of 'discipline' as 'congenitally failing'.

Sociologies of sociology

Sociologies of sociology derived from the same puzzle as this thesis. When Gouldner (1970: 55) spoke of sociologists keeping two sets of books, for example, one for their own practice and another for the social world they studied, he was addressing just the puzzling reflexivity found in the formal sociologies of knowledge. But many attempts to resolve the difficulty ended in repetition of it. From an extensive literature - Reynolds and Reynolds (1970) and Tiryakian (1971) are representative collections - attention is restricted here to Gouldner himself and to two widely cited sociologists of sociology, Friedrichs (1970) and Ritzer (1975; 1981). Each of these opened sociological knowledge to scrutiny, each used Kuhnian language, and each did so with the familiar interruption of Mertonian readings of Kuhn.

Publication of Gouldner's forecast of a 'coming crisis in Western sociology' and of Friedrichs' and Ritzer's accounts of sociological paradigms were disciplinary events: Gouldner's sparked controversy;⁵ Friedrichs' earned a Sorokin Award;⁶ and Ritzer's was one basis for his subsequent disciplinary stature. For all that Gouldner (1976: 281) was to complain of "'normal' sociology's common unwillingness to apply the 'sociology [of] knowledge to itself'", these responses confirm a reflexively processual stress in each of the three disciplinary accounts. Each was Kuhnian. After not mentioning Kuhn in *Crisis*, Gouldner (1973: 158) later identified his own use of the term - "a system in crisis may, relatively soon, become something quite different than it has been" (Gouldner 1970: 341) - with the stimulus for a shift in paradigm, and his 'dialectical' resolution of Parsonian functionalism with Marxism had the agonistic character of that mechanism. Friedrichs' model of a contest between 'consensus' and 'conflict' paradigms and between 'priestly' and 'prophetic' modes of sociology was also dialectical, albeit more in Berger and Luckmann's (1966) sense than in Gouldner's Lukacsian version. Ritzer did not pose antitheses - for him, sociology was a 'multiple paradigm science' - but he did suggest the same contested emergence as had Gouldner and Friedrichs. So, at first glance, these widely noted diagnoses of 'crisis' had the fluidity of Kuhn's early work.

But that link ended in familiar purifications. Using Kuhn's 'ideas' rather than his 'lived exemplars', each of the three writers assumed the characteristic separation

⁵ He was to talk later of "'normal' sociology's ... knee-jerk hostility to a 'sociology of sociology'" (Gouldner 1976: 281; cf. Gouldner 1973a).

⁶ Administered by the ASA, this was presented 'to the author of the publication which, in the opinion of the Selection Committee, contributed outstandingly to the progress of sociology during the preceding two years'.

of the epistemological from the political. This was in part an effect of unfamiliarity with the sociologies of knowledge. When Gouldner contrasted a stress on the *internal* in his manifesto for a reflexive sociology - effectively a call for a shift from sociology in itself to sociology for itself - with "the intellectual distortions subtly produced by *class*-rooted differences in political ideology" in classical sociology of knowledge (Gouldner 1970: 512), he could not have known the continuities between internal and external in Mannheim's questioning of epistemology. He did not mention Mannheim at all in *Crisis*, and made scant reference elsewhere (e.g. Gouldner 1973b; 1976). What at first appeared a return of the repressed in his account was a return of the repressing.

A similar effect appears in Friedrichs' and Ritzer's calls for a new sociological paradigm. While Friedrichs (1970: 10) described his reading of Kuhn as being "in the highest tradition of the sociology of knowledge and its more youthful progeny, the sociology of science", and ranked Mannheim among the disciplinary 'prophets', he repeated an orthodox caricature: Mannheim "would naively exempt the intellectual from the social conditioning that he saw dominating the ideologies and utopias offered by the bourgeoisie and the proletariat" (Friedrichs 1970: 185). In assuming that paradigmatic conflict was destructive, he repeated too the imaginary of apolitical knowledge. That appears more strongly in Ritzer's misreading of 'paradigms'. Where Kuhn had seen knowledge as constitutively political, Ritzer (1975: 203) deplored the way that sociologists "often completely forget the question of knowledge advancement in their efforts to politically advance the cause of their paradigm", and proposed a neutral 'integrated paradigm' or 'meta-theory' (cf. Ritzer 1981; 1992a). When he held that paradigmatic differences "would be far more useful if the sociologists involved would tone down the rhetoric and concentrate on furthering sociological knowledge" (Ritzer 1975: 210), he epitomised a dream of epistemological purity. 'Crisis' in that sense is a denial of what is axiomatic in the sociologies of knowledge.

Now, whatever the justice in Ritzer's (1992b: 14) description of the sociology of sociology as "highly limited navel-gazing", or in his claim that a focus within it on minutiae had "helped to give it a poor reputation", it was not sustained. But the politico-epistemological sense of 'crisis' remained, as a disciplinary common sense.

Continuing crisis

Study of 'the discipline' has never been limited to a subdiscipline; indeed, on Peek's

(1971: 447) description of the sociology of sociology as mostly unmethodical 'shoptalk', it may not be possible to talk of it as a subdiscipline at all. But whether or not they are put explicitly in terms of 'crisis', more recent diagnoses of 'the discipline' have repeated subdisciplinary accounts of it. In responding to critiques of his *Handbook of Sociology*, for example, Smelser (1989: 854) held that:

It is now almost universally accepted that the recent history of sociology is a story of an accelerating process that goes by the names of specialization, differentiation, fragmentation, and centrifugality. Tendencies to integrate and synthesize, moreover, have not kept pace, so it seems not greatly exaggerated to describe the current status as all periphery, no core.

In a widely cited account of 'the discipline', Collins (1986: 1336) likewise claimed that:

There are complaints from many directions: that the field had grown repetitive, stagnant, fragmented; that it has lost its public impact or even its impulse to public action; that it lacks excitement; that it no longer gets good students or has good ideas.

Collins distinguished in this analysis between intellectual and institutional aspects of sociology, and many recent accounts similarly favour one or the other side of this conventional split. In Wiley's (1985) 'interregnum' in American sociology, in Wardell and Turner's (1986) 'dissolution of the classical project', in Alexander's (e.g. 1988) 'new theoretical movement', in Ritzer's (e.g. 1992a) 'metatheorizing', in Seidman and Wagner's (1992) accommodation of postmodernism, in Horowitz's (1994) attack on 'subjectivism', or in Stacey and Thorne's (1985) and Ferree and Hall's (1996) calls for an integration of feminism, descriptions of and prescriptions for the discipline are couched in terms of its intellectual or theoretical state. Methods vary, from reading beyond the American tradition (e.g. Kellner 1990, Calhoun 1992a), through revival of the founding fathers and classical problematics (e.g. Alexander 1989; Sica 1988; Camic 1989, 1992; Baehr and O'Brien 1994; Wrong 1994), to citation-analytic confirmation of dispersion (Crane and Small 1992), but an agreed sense recurs: the crisis in sociology is a crisis of sociological theory and intellectual fragmentation.

Alternatively, the crisis may be institutional. Following Blalock's (1989: 457) claim that sociology "is not a high quality discipline", for example, D'Antonio (1992) related depressing trends in disciplinary recruitment to events in the ASA. Simpson and Simpson (1994: 259) have suggested that the Association is a victim of its own success:

Since the 1950s, [it] has expanded its activities beyond its original disciplinary focus. It has taken more interest in nondisciplinary activities and has expended effort and resources on them. It has become not just a body of scholar/researchers, but increasingly a professional association. ASA's emphasis on applied sociological practice has grown, as has its attention to devising formal ways to improve undergraduate teaching.

The association has also become more responsive to political interests of its membership. It has supported the pursuit of social justice in the world at large. It has taken steps to democratize its own structure, seeking more representation and participation of previously excluded or disadvantaged groups as ends in themselves. These actions embody cherished democratic values, but as organizational goals and actions they have blurred the disciplinary focus of ASA. ...

Pointing to a decline in membership of almost 25% between 1972 and 1984, a decline in commitment to voting (in 1959, 62.3% of eligible members voted, compared with 32.4% in 1992), an increase in organised sections, and a dilution of disciplinary elites, they depicted the Association as failing the discipline. Huber (1995) presented a similar picture, in setting the 'risks' facing sociology within the context of fiscal constraints; when even notable departments were being either closed or threatened, and when sociology as a whole was required to justify its support, "the politicizing of the ASA over the last two decades has decreased its ability to deal with disciplinary issues" (Huber 1995: 211).

Conveying all the purity of the distinction of 'discipline' from 'profession', Simpson and Simpson's 'disciplinary focus' and Huber's 'disciplinary issues' confirm the sense of 'crisis' in a Mertonian reading of Kuhn, or in Parsonian-Mertonian understandings of 'the discipline'. But openings of those foreclosures are also part of disciplinary common sense.

Permanent crisis

Accounts of 'the discipline' more consonant with the argument of this thesis can also be found, from during, before and after the heyday of the sociology of sociology. Treating 'theory and theory groups in American sociology' through their personal, textual and organisational networks, for example, Mullins (1973) stressed the Kuhnian politics of knowledge which Friedrichs and Ritzer had preempted. This also required departure from Merton, as when he held that members of a theory-group were not published and employed either by accident or by straightforward criteria of merit:

The social processes of science must be manipulated by someone to produce the conditions necessary for group growth. This necessity is contrary to the presumed norms of science, but it is necessary for scientific development (Mullins 1973: 305).⁷

Shils - Mannheim's translator; Parsons' collaborator - had depicted the discipline as more an ecological tradition than as an effect of normative or logical standards:

⁷ Mullins distinguished eight 'theory groups in American sociology: 'standard American sociology; symbolic interactionism; small group theory; social forecasting; 'new causal theory' (path analysis); ethnomethodology; structuralism; and radical-critical theory.

Sociology at present is a heterogeneous aggregate of topics, related to each other by a common name, by more or less common techniques, by a continuity of key words and conceptions, by a more or less commonly held aggregate of major interpretative ideas and schemes. It is held together by a more or less common tradition - a heterogeneous one in which certain currents stand out - linked to common monuments or classical figures and works... (Shils 1970: 165).

Part of that more or less common tradition is attention to sociology's political structure. Mills' (1959) critique of 'grand theory' and 'abstracted empiricism, for example, appeared at the height of the 'golden age', and, beyond his own early work (e.g. Mills 1943) he had such predecessors as Lynd (1939) or Sorokin (1956).

More recently, some writers have treated 'crisis' teratologically. Thus Wolfe (1992: 773) held that since historico-comparative work within a recovery of the classical tradition was flourishing, "[s]ociology may be in the doldrums; sociologists have rarely had it better". Stinchcombe (1994: 279, 290) saw sociology's 'disintegrated' condition, with "many different and incompatible standards for what is good work", as "the optimum state of affairs, both for the advance of knowledge and for the expansion of mind of undergraduates". 'Crisis' might even be a permanent state, for both sociology and a changing world, and 'the discipline' simply has to adapt to it (Lemert 1995). Study of that process required attention to disciplinary rhetorics (e.g. McCloskey 1986; Sica 1992), or a reconciliation of knowledge and organisation. Halliday (1992) stressed the latter in his remarks on sociology's 'disciplinarity' (in the sense of 'disciplinary identity'), as did Calhoun (1992b) on disciplinary boundaries, and Buxton and Turner (1992: 403), in claiming that "the great venture of the creation of a professional sociology was a failure, except in the realm of conventional disciplinary politics".

Turner and Turner (1990) gave an extensive account in this vein, in depicting American sociology as 'the impossible science'. They started from the same point as in this thesis, that to treat sociological knowledge philosophically is sociologically inadequate. On the further assumptions that the "structure of sociology as an academic discipline and the production of ideas is intimately connected to the nature and level of resources that have been available to sociologists", and that as the discipline emerged from the reform movements of the nineteenth century "the material, symbolic and organizational resources available to sociologists were limited and often contradictory" (Turner and Turner 1990: 8, 31), they examined the organisational preconditions of the discipline. As was especially clear under the plentiful funding from both government and private foundations which characterised the 'golden age', the conditions which

made the discipline possible were also those which precluded its consolidation around an agreed core. Whether sociologists looked to internal specialities or external funding bodies, they could always invoke sources of support other than 'the discipline'. That tension was given in sociology's institutionalisation. Even during the fiscal tightening of the 1980s, symbolic resources remained so diverse, and sociologists so regionally and sectionally divided, that no central disciplinary control was possible. Turner and Turner (1990: 197) concluded that:

Either the practical conditions for the intellectual ambitions of the discipline or the intellectual conditions for the practical institutional ambitions of the discipline have remained tantalizingly close but out of reach. These conditions are entrenched. And so, while sociology may always exist, and perhaps prosper in times of high student interest, it will remain the impossible science.

Subsuming processes given in both the sociology of sociology and sociological shoptalk, Turner and Turner's attention to 'material, symbolic and organizational resources' is also a version of the real, discursive, and collective moments of a problematic translation chain.⁸ Although it is little grounded in the sociologies of knowledge, it ends in exactly that processual tension which is missing from Mertonian readings of Kuhn and which is being imputed in the thesis to 'discipline' as a 'congenitally failing operation'. 'The discipline' matches the modelling of 'discipline'.

'The discipline' in crisis ? Reprise

The treatment of 'crisis' as an effect of the politically epistemological, and the reading of it in 'the discipline', have been couched in this section in terms of what is denied in formally Mertonian-Kuhnian accounts and in tacitly Mertonian-Kuhnian common sense. It is characteristic of both the field and Merton's place in it that that reading could also be grounded in his own work. He had suggested that in diagnosing a 'coming crisis' Gouldner had "rather understated the case. For it can be argued, without paradox and with as much persuasiveness, that sociology has been in a condition of crisis throughout its history" (Merton 1975: 21). In a paper on the sociology of sociology, where he treated relativism as a version of the Cretan paradox, he had sketched how 'the discipline' was still possible. Working scientists, he held, never found relativism a

⁸ Jonathan Turner, in particular, might not agree. Turner and Turner (1990: 8) stressed their own differences on "the feasibility of a scientific sociology". But, they added, "whatever our philosophical disagreements, we agree that the organization of sociology as a whole hinders its development as a science. For J. H. Turner, this situation represents a great tragedy, whereas for S. P. Turner such histrionics seem unwarranted".

difficulty, for they have “repeatedly - one might say, constantly - dealt with ... [it] in much the same fashion. Pragmatism rather than strict logic takes over” (Merton 1971: 196). But while he quoted James on the way rationalists use relativism as a ‘mechanical toy-target’, and thus effectively denied his own earlier epistemological critique of Mannheim - “[t]he Cretan paradox, threatening as it is in the abstract, will be consigned to oblivion in sociology, just as it has been in other sciences, as sociologists get on with actual investigation” (Merton 1971: 197) - he never revised his sociology of knowledge in those terms. In a typically circular effect, Merton then exemplified what he diagnosed, enacting the ‘failing’ common to ‘discipline’ and ‘the discipline’.

‘THE DISCIPLINE’ AND THE PRESIDENCY OF THE ASS/A

The presidency of the ASS/A is a peculiar office. Elected within an ever-democratising organisation, the presidents are, as Goffman (1982) said, ‘led to feel that they are representative of something, and that this something is just what their intellectual community wants represented and needs representing’. But ‘this something’ remains elusive. The presidency might be part of the ‘reward system of sociology’, in Merton’s sense, but since ‘sociology’ has all the uncertainty of the Cretan/Mannheimian paradox, so too does what is rewarded within it. After a note on organisational aspects of the presidency, meanings imputed to it will be drawn from remarks by the presidents themselves, and from the assumptions, asides and direct treatments in a range of disciplinary texts. It will be seen that the relation between sociological knowledge and sociological rewards is more contingent than Merton’s model can accommodate, but that it is amenable to fractal ordering.

The Presidency and Formal Organisation

The presidency has been a prescribed constitutional office since formation of the ASS. As noted earlier, the first five presidents were elected for two-year terms, but the office was soon set at one year, and reelection is debarred. The franchise is restricted. Degrees of membership in the ASS/A, and the voting rights attached to them, have been adapted throughout its history; currently student and associate (including foreign) members, who together make up about one third of the membership, are not eligible. Voting was also limited for a time to those attending the Annual Meeting, but since the

1940s election has been through postal ballot. Forms of nomination for office have also varied. Until the reform of the 1960s, two candidates for the presidency were selected each year by a Committee on Nominations (after, in principle, wide consultation), who in turn were appointed by each incoming president; membership of the Committee is now itself an elected office. 'Petition candidates' may also be nominated independently of the Committee if enough members write their names on the ballot papers; run-off elections may then be required.

As nomination and election have become more decentralised, so the presidents' rights and duties have become more limited. The presidents are *ex officio* members or chairs of various committees within the ASA, but given both the formal apparatus of the Association and the role of the executive committee they may have little to do with administration. Would-be activist presidents have had little success (cf. Faris 1981), and any number of presidents have noted that their principal duty is the setting of the theme for the annual conference and the arranging of sessions around it.

But the presidency cannot be divorced from organisational functions, even if expectations vary. This can be illustrated through presidential reports during the 'crisis' and constitutional revision of the 1960s. Writing in a "year of turmoil", Hughes (1962: 902-3) noted that:

Some have suggested that the President devote a year to the administration of the Association, to representing it before the regional societies, sister learned societies, the public, and the government. Others - and I am among them - believe that the Presidency should be - and it is - a high honor conferred by one's colleagues upon one who may, in rare cases and quite incidentally, have or have had in his youth, some penchant for administration or public relations, but who may also never have had those qualities.

In his observation of 'turmoil', Homans (1964: 895) implied a failure of representation:

Certain views disturbing to the President have been current among the members, ranging from the view that the Association is dominated by an 'Eastern clique' among its officers to the probably more realistic view that the officers have been somewhat out of touch with the regional societies.

But where Homans gave qualified support to the establishment of the ASA's executive in Washington DC, for the access to governmental funds and information it allowed, Sorokin (1965/6: 34) warned that "an increasing concentration of power" in the executive apparatus tended "to make the elective offices of the President and the Vice-President purely honorary with little influence in determining the policies of the Association". Moore (1966: 279), however, in "the political year", seemed to find the office far from purely honorary: "I have had the exceptional good fortune ... of an organizational

setting in which I could do almost adequately almost all of the tasks that were almost necessary. It is too much". But two years later, when the constitutional reforms had been enacted, Hauser (1968: 326-7) found that:

It has definitely become easier for the Presidency to be essentially an honorific rather than administrative office; and the decentralization of decision-making through the Council and the various Constitutional Committees effectively prevents the elected officers from exercising undue influence on the affairs of the Association.

Entangled expectations - over representation, policy-making, administration, and honour - are evident in those reports, and they are drawn from just one period of 'crisis' in 'the discipline'. The same effect is found in uses of the presidency as symptomatic.

The Presidency as Documentary of 'The Discipline'

The presidency has often been used to document the state of 'the discipline'. Odum (1951), for example, himself a former president, took the presidents of the ASS as representative of American sociology, as did Armer and Grimshaw (1973) in their indexing of the rise and fall of cross-national comparisons. More recent writers have treated the 'dilution of elites' evident in the presidency as indicative of disciplinary decline (e.g. Wolfe 1992; Simpson and Simpson 1994; Huber 1995).⁹ Less analytically, when the comments of former presidents were used to mark the ASA's seventy-fifth anniversary the presidency was treated as organisationally representative (e.g. Faris 1981). But given the emergent character of a discipline, the presidents are formal representatives of shifting representations. The difficulty of fixing meanings within such a swirl was stressed in Chapter 4. Crane (1965: 703), for example, was noted there as indexing academics' 'eminence', 'prestige', 'influence', or 'power' through their holding of office in a national association, but as conceding that since this recognition was "more likely to be obtained by an 'operator' than by a 'scholar' ... it may measure 'success' more than 'merit'". Like 'crisis', this has become something of a disciplinary trope. Huber (1995: 211), for instance, claimed that "more and more ASA officers appear to have been nominated for reasons other than eminence in the discipline", and Wolfe (1992: 768-9) that "[h]onor in the profession is more politicized, as candidates write position statements, appeal to various constituencies, and are rewarded for their

⁹ e.g. "between 1950 and 1969, eight of the twenty presidents ... were affiliated with the four major universities. ... The entire decade between 1975 and 1985 produced not a single president from the top four departments. ... To the degree that presidency of the ASA represents a judgment about the honor of individuals (and the departments to which they belong), something happened in the later part of the postwar period to broaden the honor" (Wolfe 1992: 768).

networking as much as for their scholarship”.

Those processes may have become more overt, but their recency is another question. The few formal studies of the presidency suggest that the relation between disciplinary honour and scholarly eminence is both epistemological and political. In a study of individual self-justifications and collective legitimations, for example, Westie (1973) asked respondents in the discipline to rate their familiarity with the name or work of a number of sociologists, who, without his respondents being informed, were all the presidents of the ASS/A from Ward in 1905-6 to Goode in 1972.¹⁰ Westie was then able to rank the presidents’ ‘disciplinary survival’. The extent to which many had been forgotten suggests that election to the presidency is a poor predictor of ‘merit’.

While a converse study would be interesting - of eminent sociologists who did not become president - none such has been found. But two analyses of voting in the ASA’s presidential elections do confirm the contingency of disciplinary recognition. Following a finding on American politics generally, that propensity to vote is correlated with position in the social structure, Ridgeway and Moore (1981: 74) hypothesised that voting in the ASA would be “associated with a member’s position in the status structure of the profession and the organizational network of the association”. Their results confirmed that those with the greatest ‘investment’ or ‘interest’ in sociology would be more likely to vote, for the more established members did so disproportionately: the middle-aged; men; the higher professional ranks; those associated with prestigious institutions; and those engaged in the structure of the ASA. They concluded, somewhat contradictorily, that “despite the honorific non-political quality of ASA presidential elections, voting in these elections does reflect more complex social processes than the simple recognition of sociological achievement” (Ridgeway and Moore 1981: 81). When D’Antonio and Tuch (1991) replicated the study on two later elections, they found that academic employment, rank, age or gender had no significant effect on propensity to vote, and so did not support Ridgeway and Moore’s ‘investment’ hypothesis. But since affiliation with either one of the ASA’s sections or with Sociologists for Women in Society did increase a member’s odds of voting, they concluded that disciplinary ‘networks’ affected the turnout of voters. In either case, then, the presidency was seen as an effect of ‘more complex social processes’.

¹⁰ Fractal entanglements are pervasive. Westie was a student of Wolff, who had been a student of Mannheim. His study of the ‘social construction of reality’ (in Berger and Luckmann’s (1966) sense) was itself first presented as a presidential address, to the Ohio Valley Sociological Society.

Johnston (1987) highlighted such processes in his study of one of the ASA's most overtly 'political' presidential elections, that of Sorokin (1965). After losing narrowly to Znaniecki (1954), and being "isolated, discredited and disliked" (Johnston 1987: 109) within the discipline, he had never been re-chosen by the Committee on Nominations. In the early 1960s, however, a campaign on his behalf began with the circulation of an open letter, from a Committee of Eight organized by Otis Dudley Duncan and Beverly Duncan. Invoking the provision for popular nomination, the Committee requested that Sorokin's name be written-in should the Committee on Nominations repeat "the pattern of recent years ... [and] overlook one of our most distinguished members" (in Johnston 1987: 109). No candidate had ever been written-in by the required 10% of the voting membership since the introduction of postal voting, and there had never been such open campaigning. The Committee protected itself against possible controversy in two ways. It was composed of eminent figures within the discipline, including two past and two future presidents - Blumer and Merton, Loomis and Sewell - and, on the grounds that the presidency had meaning only if it reflected a broad consensus, was presented as campaigning for Sorokin's *nomination* rather than for his *election*. As Duncan wrote:

While our procedure for nominating Sorokin is sanctioned by the law-norms of the American Sociological Association ... I think that any organized 'politicking' about the election as such would be beyond the conventions now considered proper. No one openly electioneers or has electioneering done for him. Our position has been that the membership has been thwarted by a succession of nominating committees that has refused to give Sorokin an opportunity to be elected (in Johnston 1987: 116).

Using a rhetoric of justice - Sorokin had been denied the second chance which by custom followed a sizable vote on first nomination - the Committee induced a clarification of perceptions of the presidency. It was not an administrative position, but was "the highest honor that we as a group can bestow" (in Johnston 1987: 113). Or, concern that Sorokin's election would mar the scientific standing of sociology through an appearance of endorsement of his religiously infused work was countered with the claim that "[b]y honoring a man for past contributions we do not thereby take his theories as an orthodoxy" (in Johnston 1987: 114).

As it eventuated, Sorokin was not nominated, over 10% of eligible voters wrote-in his name, and he won the run-off election over Moore (1966) and Arnold Rose.¹¹ The 'apolitical' nature of the presidency suggested by Ridgeway and Moore (1981) can be sustained only on a narrow definition of 'political'. Disciplinary honour appears an

¹¹ Rose was elected to the presidency for 1969, but died before he assumed office.

networks, as in the Committee of Eight.¹² The link with disciplinary knowledge is as contingent as Westie (1973) found or as Crane (1965) implied in her distinction between 'merit' and 'success'.

'The Discipline' and the Presidency of the ASS/A : Reprise

The presidency of the ASS/A may or may not be more ceremonial than administrative, more an honour for past work than a vehicle for disciplinary reform, more a recognition of eminence than a token of intellectual agreement, or more a mark of political success than of epistemological purity. Since any of those possibilities were given in the remarks and studies noted here, investigation of the presidency as part of a Mertonian 'reward system' of sociology is problematic. What representatives of 'the discipline' represent remains uncertain.

One point, however, is agreed: each president is responsible for the programme of the annual meeting. As Hughes (1962: 903) said:

In my opinion, the President should be so free of administrative detail that he could devote the year to encouraging his colleagues in the planning of a program of high quality, and to preparation of a presidential paper which might be an intellectual ornament, and, if he be the kind of man who can do it, a paper which may open up new fields of thought and research.

The meaning of such a 'presidential paper' must be as contingent as the presidency. But since addresses might be expected, too, to be generically distinct fusions of the forensic, epideictic and deliberative rhetorics of Hughes' 'encouraging', 'intellectual ornament' and 'new fields', they are documentary documents of 'the discipline'.

'THE DISCIPLINE' AND PRESIDENTIAL ADDRESSES

As an institutional feature of 'the discipline' since its formalisation, and of the ASR since its foundation, presidential addresses allow a regular view of the ceremonial face of 'the discipline'. Each address is delivered after the Annual Dinner, and each is published as the lead paper in a subsequent issue of the *Review*. They are generically distinct - as Goffman (1982) said, a 'presidential address faces one set of requirements, an article in a scholarly journal quite another' - even if that distinctiveness is not

¹² e.g. Otis Dudley Duncan was the son of one of Sorokin's early friends and colleagues; Merton and Loomis were both former students.

immediately apparent. The addresses have the same apparatus of abstracts, bibliographies, and footnotes as do standard articles, and show the same trends of increasing length and more detailed referencing.¹³ Indeed, while most now appear to be little read, some have been absorbed as standard articles, being routinely cited without mention of their occasionality.¹⁴ Their 'set of requirements' will be introduced here through previous accounts and through a recapitulation of the four case-studies thus far. In the opening of Mertonian sociology of science through repertoires and rhetoric, the addresses' generic distinctiveness will be shown as a fractal ordering, and the addresses themselves then as a folk sociology of knowledge, distinguished from the formal sub-/metadiscipline by scant references to it. That sets the antimetabolic terms of the thesis.

Presidential Addresses as Documentary of 'The Discipline'

Presidential addresses have been little studied. Only two earlier analyses have been located, one very brief, and these are more of interest for occlusions than for inclusions; in both, a suggestive stress on the addresses' occasioning is preempted by the separation of the political from the epistemological. In the first, Kubat (1971: 2) introduced a collection of all addresses from 1946 to 1969 with the claim that they offered:

first hand access to the minds of those... chose[n] to represent the sociological body politic to the other disciplines, to the society at large, and first and foremost to the membership of the Association.

He typologised the addresses by whether they were directed to either a better society or a better discipline, through either knowing or doing - that is, as concerned either with analysis of social change and recommendations for policy or with sociological theory and methodology - and concluded that the roughly equal distribution of the addresses among the four cells of his typology showed "the maturity of the discipline which has neither to demonstrate its *raison d'être* nor to mend incessantly its fences" (Kubat 1971: 3). In the second study, and in contrast with that optimism, Kinloch (1981: 2) read the addresses from 1906 to 1979 as evidence that American sociology was "the

¹³ From 1935 to 1944, the mean length of the addresses was 5,300 words, and the mean size of the (implicit) bibliography was 16.4, while the corresponding figures for 1985 to 1994 were 8,900 and 62.7. Those increases are comparable with the findings in Lovaglia (1991).

¹⁴ On impressionistic readings, these include Sutherland's {1939} address on 'White-collar criminality', Blumer's {1956} on 'Sociological analysis and the variable', Merton's {1957} on 'Priorities in scientific discovery', Davis' {1959} on 'The myth of functional analysis', Homans' {1964} on 'Bringing men back in', Goffman's {1982} on 'The interaction order', Short's {1984} on 'The social fabric at risk', and Lipset's {1993} on 'The social requisites of democracy revisited'.

conservative reaction of middle-class liberals to the social heterogeneity of a society undergoing extraordinary levels of industrialization and urbanization". Finding that the presidents' "basic solution to social problems is normative integration, brought about through the sociological understanding of social structure" (Kinloch 1981: 11), and that they had excluded political economy from their responses to social problems, he concluded that as merely normative projections the addresses revealed fundamental problems in the discipline.

Both these studies are flawed. Kubat's typological distinctions are familiar from the sociologies of knowledge, and as evident in his allocations of the addresses to one or another cell, they preempt in a familiar way the implications of his tropes of 'represent' and 'body politic'.¹⁵ Kinloch's study was just as problematic in design and execution, and just as preemptive of the political structuring he had stressed.¹⁶ Apart from the potential contradiction in his description of sociology as the 'conservative reaction of middle-class liberals', he left the status of the presidency uncertain when he held that the addresses:

do not reflect the structure of American sociology as a whole; rather they reflect the limited concerns of a restricted number of sociologists in leadership positions with particular backgrounds. Furthermore, presidential addresses represent an institutionalized ritual in which the speaker is expected to expound broadly on the present status and future prospects of sociology rather than providing a detailed analysis of the state of the discipline (Kinloch 1981: 3).

While 'leadership', 'represent', and 'institutionalized ritual' all imply a disciplinary body politic, the interaction which might be expected from it is missing.

In that sense, Kubat and Kinloch can themselves be read as exemplifying what is at issue here. Since the former's complacency at the maturity of sociology and the latter's outrage at sustained failure both entailed imaginaries of collectively and

¹⁵ He located Hughes' {1963}, Loomis' {1967} and Hauser's {1968} addresses, for example, in his category of 'Towards a better society - What is to be done'. While all were indeed concerned with reform, as good a case could be made for their inclusion in either cell of 'Towards a better sociology'. The converse argument could be mounted for, say, Taylor's {1946} address, which Kubat placed in 'Towards a better sociology - What is to be done'. The typology cannot accommodate the addresses' multi-faceted character.

¹⁶ His allocation of each address to one of five categories - social change, conflict, affective phenomena, the state, and the profession itself - required the same eliding of multi-facetedness as in Kubat's two-by-two typology. In his actual analysis, one president, Angell {1951}, was omitted altogether, and another, Cottrell {1950}, mistaken for William F. Cottrell, who was never president. Small's address of 1912-13 was analysed twice, while that of 1913-14 was omitted. Three presidents, Ross {1915-16}, Lichtenberger {1923}, and Merton {1957}, were listed in the bibliography but not treated in the analysis. Given the period of his coverage, 1906 to 1979, inclusive, he should have had 74 addresses, yet in his numerical analysis he gave N=70, a figure which matches neither the addresses he claimed to have considered nor those he actually did.

scientifically warranted usefulness, they epitomise the 'disciplinarity' and 'discipline' of the fractal model. But these disciplinary moments require supplementing with the 'disciplining' elided in their quarantining of the epistemological from the implications of their political tropes. So while the studies are suggestive - Kubat's (1971: 2) claim that the addresses allow "access" to the presidents is to the point, as is Kinloch's (1981: 3) that they "provide insight into the major definitions of professional sociology over a significant period of time" - they have the limits of purification. But if the addresses are not amenable to study on these Mertonian assumptions, they are still Mertonian 'strategic research materials', and the elements of a rhetorical alternative to study of the 'reward system' of sociology have been presented in the four case-studies thus far.

Presidential Addresses as Folk Sociology of Knowledge

The addresses in the case-studies were selected on two grounds, for their reflexivity, as seen particularly in Goffman's and Lazarsfeld's, and for their emergent fit with fractal ordering, as shown especially in Wirth's, Gamson's and Gans'. Through the first, the addresses has been depicted as generically entailing disciplinary honour, disciplinary policing, and disciplinary exemplification, and, through the second, as consistent with disciplinarity, disciplining, and discipline. The two triads are mutually fractal, with the collective renewal common to them being the process Kubat and Kinloch preempted. Presidential addresses are *performative* representations, and as such they amount to a folk sociology of knowledge.

Goffman and Lazarsfeld both suggested the 'set of requirements' fulfilled in an address. When Goffman dramatised the self-indulgence of presidents of professional societies, the ritual of their disciplinary representation, and their claims of relevance to events of the day, or when he described his own address as 'not being particularly autobiographical in character, deeply critical of established methods, or informed by a concern over the plight of disadvantaged groups', he implied a generic trialism. The same form appeared in Lazarsfeld's undertaking 'to trace back some of the things you know I stand for' through a fusion of the 'specific sociological problem' and the 'kind of state of the union message' he adapted from the early presidents of the ASS. Elements of what Goffman and Lazarsfeld implied were found in the studies of the presidency, for the office was taken variously as ceremonial and as organisational, as an honour for work accomplished and as a focus for work to be done, as a recognition of disciplinary

eminence and as a sign of disciplinary consensus. All that is included in the presidency as representative, and since the addresses are effects of it, must be expected in them as well. So on the assumption that fractal trinitarianism suggests both generality and completeness, the addresses can be generically distinguished as an interaction of disciplinary honour, disciplinary policing, and disciplinary exemplification. Such a representation accommodates Kubat's and Kinloch's political imagery, but leaves open what they had preempted.

A similarly processual sense was found in the reading of Wirth's, Gamson's, and Gans' addresses. Sociologies of knowledge were shown in Part I to be representations of disciplinarity, discipline, and disciplining, and it was suggested that any ordering matching that model could be taken as at least a tacit sociology of knowledge. Wirth and Gamson both presented optimism and failure in their morally framed addresses on collectivity, and Gans' treatment of 'the discipline' was even more consistent with fractal ordering. Again, the trinitarianism opens Kubat's and Kinloch's foreclosures on the political.

Given the fractal relation between those two pairs of trinary orderings, the generic requirements of the addresses make them sociologies of knowledge. Further, the finding in each case-study of a blunted reflexivity makes them a folk version of the sub-/metadiscipline. In a practical demonstration of 'Mannheim's paradox', the presidents are constrained from meeting the requirements of the occasion with formal rigour. Since rhetorical success requires the gaining of an audience's consent, the lack of an agreed disciplinary core limits the extent to which the presidents can sanction departures from their versions of it. Goffman might have described an address as a 'preachment', and Wirth may have held that a president 'is even permitted to preach a sermon', but there is little scope for a Savonarola. Restricted to appeals to common sense, the presidents are then situationally debarred from the situationally required. The disciplinary policing, or disciplining, which adds a processual sense to Kubat's and Kinloch's foreclosures, and thus allows treatment of the addresses as sociologies of knowledge, is then also the process by which formal and folk sociologies are distinguishable.

That they are distinct is seen in the scant cross-referencing between them.

Presidential Addresses and Formal Sociologies of Knowledge

Formal subdisciplinary study of knowledge and the presidential addresses under study

are linked through the coincidental events of 1936. In that year, *Ideology and Utopia* was translated and Merton completed the pioneering sociology of science of his thesis; in that year, foundation of the ASR symbolised and achieved what was at once a broadening and a concentration of American sociology, and publication of the addresses has been a feature of it ever since. For the roundness of the number, those under study are the first sixty, from Chapin's of 1935 to Gamson's of 1994.¹⁷ Now, whether or not the timing of their emergence was a Mannheimian 'no coincidence', a gap appears between the two disciplinary genres. Addresses have not been studied in terms of the sociologies of knowledge, and those sociologies have been little used in the addresses. Once that last is shown, the basis for an antimetabolic reading will have been set.

Although its publication was a disciplinary event, *Ideology and Utopia* was rockily received by American sociologists (Nelson 1990; Kettler and Meja 1995), and the sociology of knowledge never became a major focus in the discipline. In her survey of specialities within the ASS, for example, Riley (1960: 924) found that in both 1950 and 1959 only 1% of the membership claimed competence in it. That response is evident in the addresses, for only Gans (1988) explicitly linked his address to formal study of disciplinary knowledge. Presidents such as Wirth, Parsons, Znaniecki, Merton, Becker, Sorokin and Coser had published in the field,¹⁸ but references in the addresses either to the sociology of knowledge or to Mannheim are scant, and when they do appear they are more asides than focal. In talking of shifts in the study of race, for example, Frazier (1948) noted a 'growing interest of American sociologists in the sociology of knowledge', but beyond 'tentative hypotheses' for testing 'by those who are interested in this branch

¹⁷ Institutionalisation entails the smoothing of anomalies, for not all addresses were delivered as such. Although war-time restrictions on travel resulted in the annual meetings of 1942 and 1944 being cancelled, Sanderson's {1942} and Vance's {1944} papers were still published as addresses. Two presidents, Becker {1960} and Goffman {1982}, died during their terms of office. Becker's address was assembled from his notes, and was introduced and read by his son. Goffman had prepared his, but was too ill to preside over the annual meeting at which he would have delivered it. Again, both papers were published as presidential addresses. As noted earlier, Arnold Rose died between his election and his scheduled assumption of office and so was never officially president; under the ASS's rules, Turner, the vice-president-elect, was appointed in his place. The paper Rose prepared as his address was subsequently published. Kubat (1971) included it in his collection, while Kinloch (1981) omitted it from his study. Although it is also excluded here, Rose's (1969: 630) concluding point is worth noting: to study different kinds of the 'sociological imagination' he had addressed, he called for "development of a sociology of sociology. Rather than have specialists in this, who might be as unfortunately unproductive as most 'sociologists of knowledge' have been, I suggest that all sociologists of imagination devote a corner of attention to these matters".

¹⁸ Four of the 38 reviews in English of *Ideology and Utopia* which Nelson (1990) listed were by future presidents of the ASS/A: MacIver {1940}, Merton {1957}, Becker {1960} and Homans {1964}.

of sociology' he did not treat either the discipline or the occasion in its terms. Nor did Yinger {1977}, after he had claimed in his address on counter-cultures:

The way was paved for the countercultural epistemology of our time by the long tradition of unmasking in European and American thought ... Do not trust appearances or presumed objective truth, for there are deeper realities, say Hume, Marx, Nietzsche, Freud, Pareto and many others. Truth requires that we bring these deeper realities to light. If this unmasking laid the groundwork for the sociology and psychology of knowledge in the thought of some persons, it supported the search for truth through mysticism and the occult among others for whom science itself has been unmasked. (My statement itself, of course, is an hypothesis in the sociology of knowledge).

The foreclosure on 'Mannheim's paradox', implied in Yinger's link from the questioning of science to the irrational, recurred in Coser's {1975} contention that it was 'axiomatic among sociologists of knowledge that the origin of ideas does not prejudice their validity'. Sorokin {1965} did allow for alternative assumptions, in his reference to:

two kinds of the theories of *Wissensoziologie* - one taking as independent variable the category of social groups to explain cultural systems and congeries, the other taking the cultural category as independent variable and the social as dependent variable.¹⁹

But all that is scant use of a sub-/metadiscipline which, at first glance, would seem suited to occasioned requirements. As for Mannheim himself, apart from Yinger's {1977} reference to his sociology of knowledge, he was mentioned by Hauser {1968} for his work on social planning, by Riley {1986} and Lieberman {1991} for that on 'generations', and by Moore {1966} to distinguish his usage of 'utopia'; what was otherwise a recurrent silence in the addresses was made explicit in this, for although fifteen of the sixty presidents used 'utopia', none did so in Mannheim's sense of it as a performative imaginary. So neither the sociology of knowledge nor 'Mannheim' as a macroterm for it appear with any frequency in the addresses. But at the same time, the presidents are situationally required to present what Lazarsfeld called 'a kind of state of the union' account of the discipline, and that implies a doing if not an avowal of a sociology of knowledge.

The presidents' following of that generic requirement without reference to the problematics of a formal sociology of knowledge guaranteed that they retraced moves within it. Since that will be elaborated throughout Part III of the thesis, the effect is shown here through a single passage, taken from Queen's {1941} questioning whether sociologists could 'face reality' at a time of crisis; it exemplifies the disciplining of 'collectivity' over the relation between 'science' and 'usefulness'. After denying that the discipline as such should be involved in wartime policy-making, Queen added:

We have no means of learning at this moment just what motives activate those who

¹⁹ Sorokin was the only president to mention Scheler, if only in an aside.

urge direct participation in national affairs by the American Sociological Society. Some, I am sure, have an honest conviction that our professional group possesses a body of invaluable knowledge and essential skills which are greatly needed by the nation but are in grave danger of being overlooked. Others may be suspected of seeking to utilize the national emergency for the feathering of their own nests. Still others are perhaps men and women who, lacking personal recognition and security, identify the status of our craft with their own unrewarded talents.

Queen was no Marxist, but the limits of his imputation of personal 'interests' in the production and use of knowledge were precisely those which Mannheim had treated in generalising ideological analysis or which Merton had questioned in his representation of the 'complex of social trends' in seventeenth-century science. Queen's psychologised linkage of base and superstructure was a common sense sociology of knowledge.

Presidential references to the sociology of science have a rather different effect, for in them the relatively natural disciplinary worldview appears to be drawn from the subdisciplinary. It was noted in Chapter 2 that while Merton pioneered the field, in his thesis and in his papers on 'the norms', the sociology of science did not flourish until Kuhn's work was assimilated to his, and, being concentrated in the later addresses under study, the pattern of reference to the field is consistent with that. Although none of the numerous citations of Merton are for his sociology of science, and although Kuhn is never named at all, the subdisciplinary enrolments of them appear to be taken for granted. Coser (1975), for example, grounded his critique of ethnomethodology and path-analysis in Crane's (1972) Kuhnian-Mertonian work; Peter Rossi (1980) drew on the same assumptions when he '[left] it to the sociologists of science to provide an understanding of why we have such mixed feelings toward applied work'; and so did Gans (1988) in holding that 'we know full well, in part from research in the sociology of science, that natural scientists do not operate according to the idealized conception of their method'. The term 'paradigm' seems to have been absorbed into the disciplinary language, as when Whyte (1981) noted that 'those attached to the established paradigm tend to ignore the facts that don't fit or to make patchwork adjustments in the theory', or when Lieberman (1991) referred to 'nonproductive paradigm battles'. Having a familiar normative form, the subdiscipline had a consonance with the discipline not found in the sociology of knowledge. Only Lee (1976) struck an egregious note, when, in calling for the freeing of sociological research from organisational constraints, he endorsed the statement of 'a wise modern chemist' that:

individual success in research is associated with 'a shedding of any undue reverence for embalmed science of the past'. This chemist contends 'that if one wanted to become a

creative chemist one should certainly learn all he could about chemistry. He should then decide to refuse to believe any of it. From then on he would be free to select on the basis of his own thinking the relevant ideas and reject the irrelevant'.

Already suspect in Merton's account, that romantic image of the scientist had been even more strongly questioned by Kuhn. Lee was then the exception proving the rule, and the rule was a Mertonian-Kuhnian taken-for-grantedness. But despite that difference in disciplinary common-sense, the sociology of science was no more turned systematically on either 'the discipline' or the occasioning of the addresses than was the sociology of knowledge. A rift then appears between formal study and the folk sociology of disciplinary knowledge found in these disciplinary documents.

'The Discipline' and Presidential Addresses : Reprise

Presidential addresses generically entail disciplinary exemplification, disciplinary honour, and disciplinary policing; that fractal ordering makes the addresses a sociology of knowledge. Further, since disciplinary rhetorics require both reflexivity and a constraint on it, that sociology is a folk version of the sub-/metadiscipline; that claim is provisionally confirmed through the scant reference in them to formal study. Merton has again been enrolled here against himself. The addresses are Mertonian 'strategic research materials', but they are more amenable to fractal ordering than to the preemptions in his own work and in the disciplinary common sense it represented.

AMERICAN SOCIOLOGY AND PRESIDENTIAL ADDRESSES: REPRISE

This textual ethnography of 'American sociology' was to be a test of fractal ordering against Mertonian sociology, a demonstration of the antimetabolic relation between formal and folk inquiry, and a setting of the context for further study of the latter. Each objective has been met. 'The discipline' fits the modelling of 'discipline', presidential addresses have been shown as folk sociologies of disciplinary knowledge, and a context for their reading has been set.

After a note on the organisation of 'the discipline', the chapter was developed through three narrowing foci. First, the very fact that despite agreement on its formal history critical treatment of American sociology is both so extensive and so diverse is *prima facie* evidence that 'the discipline' shows the perennially failing character of

'discipline'. As seen in use of the trope of 'crisis' in formal sociologies of sociology and in sociological shoptalk, that entails the Mertonian distinction of the epistemological from the political senses of representation. But fractal ordering could also be grounded in 'the discipline'; it is consistent, for example, with the interactive tensions subsumed in Turner and Turner's (1990) depiction of American sociology as 'the impossible science'. Merton's own 'failure' to reconcile his pragmatic resolution of 'Mannheim's paradox' with his earlier preemption of it was used to fit 'the discipline' to the 'failing' of 'discipline'.

Merton's lead was followed again to narrow the focus on 'the discipline'. The presidency of the ASS/A was taken in his sense to be part of the 'reward system' of sociology. Again, since the presidency was found to involve an uncertain representation of disciplinary honour, disciplinary policing, and disciplinary exemplification, fractal ordering allowed more complex treatment than in Mertonian equations of rewards with the epistemologically warranted.

More narrowly still, presidential addresses were taken as Mertonian 'strategic research materials', and were shown to be generically distinct effects of the three moments of the presidency. Amenable to fractal ordering, and restricted by disciplinary common sense in a rhetorically generated version of 'Mannheim's paradox', the addresses then amount to a folk sociology of disciplinary knowledge. That sets the antimetabolic terrain of the thesis: the sociologies of knowledge are representations of 'discipline' and representatives of 'the discipline' act as sociologists of knowledge. That in turn resolves what at first seems puzzling, that presidents who had written extensively in the sociology of knowledge did not use its resources in their required examination of 'the discipline'. They did not need to. They had available an alternative form of disciplinary rhetorics, but an alternative which will be seen in Part III to be antimetabolic rather than antithetic.

As a survey of (pre)texts and (con)texts on the assumption that 'the discipline' is 'narrated like discourse', the test of fractal ordering throughout the chapter has itself been part of a fractal ordering. After a note on the generic rhetorics in Coser's [1975], Blau's [1974], and Parsons' [1949] addresses, that ordering will be extended; 'the discipline' will be treated in the next chapter as 'real like nature'.

CASE 5 : COSER'S, BLAU'S, AND PARSONS' GENRES

Derived in part from the forensic, deliberative, and epideictic genres of classical rhetoric, 'fractal ordering' was developed to replace the preemptions required of typological categories with the interaction given in the element common to the three genres, the crystallisation of inchoate consensus. The extension from generic typology to 'fractal ordering' will be illustrated here through a reading of Coser's {1975} forensic philippic against ethnomethodology and path-analysis, Blau's {1974} deliberative outline of 'parameters of social structure', and Parsons' {1949} epideictic apologia for 'the prospects of sociological theory'. While the addresses can be fitted to the genres, the implications of their common occasioning are more amenable to study through the trans-scalar interaction of fractal ordering.

Coser as forensic

Coser's address on 'the serious problems now faced by our discipline' is forensic, being both combatively policing in content - 'this Presidential Address is an exercise in the uses of controversy' - and normative in structure. Treating sociology through Crane's (1972) model of disciplinary growth, in which a pioneering phase is followed first by authoritative consolidation and then by decay, Coser held that while it was largely in the second phase, signs of decadence were emerging: in 'the recent insistence among many sociologists on the primacy of precise measurement over substantive issues', as in path-analysis; and in an 'exclusive insistence on one particular dimension of reality and one particular mode of analysis by cliques or sects who fail to communicate with the larger body or with one another', as in ethnomethodology. Although the two methods were dissimilar, the 'hypertrophy of method at the expense of substantive theory' common to them was 'an expression of crisis and fatigue within the discipline and its theoretical underpinnings'.

Coser charged many path-analysts with the 'fallacy of misplaced precision'; a powerful method was abused when indicators could not be well defined. The technique was attractive, he said, since 'under the pressure to publish to avoid perishing, or to gain promotion, or simply to obtain the narcissistic gratification that comes from seeing one's name in print', sociologists were tempted to use methods guaranteeing quick and impressive-seeming results which did not require hard conceptual work. The fault was

not in the method itself. Rather, abuse of it was 'a temptation for lesser minds. And here as elsewhere inflation has set in'. As an example he took studies in stratification. Path-analysis at its best had been used to illuminate the effects of social inheritance and personal effort in the attainment of socio-economic status, but many results were trivial, and even success had entailed neglect of structures of power and exploitation. In concentrating on 'making it', path-analysts had ignored the effects of class. The method, then, had not led to theoretical development proportionate to its use.

While Coser faulted the excessive and misplaced use of path-analysis, he saw ethnomethodology as an affront in itself. It was 'aggressively and programmatically devoid of theoretical content of sociological relevance', even as its practitioners 'claimed access to types of knowledge not accessible to the sociological *vulgus*'. It was a sect rather than a sociological specialty, comprising 'a small number of practitioners huddled around a charismatic leader and his apostles', whose findings generally had an 'embarrassing triviality' and whose writings were 'mostly dross or interminable methodological disquisition and polemics'. Again Coser used stratification to suggest decay. Ethnomethodologists' refusal to research socio-economic effects resulted in 'a massive cop-out ... an orgy of subjectivism, a self-indulgent enterprise'.

In the sixty addresses, no other negative sanctioning is so direct and personal. This conventional disciplining is reinforced structurally, for in depicting path-analysis and ethnomethodology as apparently dissimilar but complementary pathologies, Coser used a standard rhetorical scheme, antithesis.¹ This implies and elicits consensus, not quite independently of content, but complementarily to it. Whether through binary contrast, the ranking of which is shared by rhetor and audience and the effect of which is to praise one pole by condemnation of the other, or through an enthymemically dialectical resolution of opposed theses, it is effective in itself in prompting consent. Primed by Coser's antithetical introduction, that shifts in the discipline 'seem to foster the growth of both narrow, routine activities, and of sect-like esoteric ruminations', his audience/readership is on familiar ground. His layered oppositions - path-analysis and ethnomethodology were both contrasted with the mainstream, while through the more personalised attacks on the latter the sins of the former were tacitly presented as less grievous - then result in themselves in a policing effectivity.

¹ It is a nice touch that this has been confirmed from ethnomethodological study to be one of the commonest techniques for signalling an audience that applause is due (e.g. Atkinson 1984; Heritage and Greatbatch 1986).

The tone of Blau's address the previous year had been quite otherwise. Where Coser was florid and forensic, Blau was deliberate and deliberative.

Blau as deliberative

Like Coser, Blau made his intentions clear in his opening sentence. Continuing his earlier macrostructural work on bureaucracies and exchange, he couched the address as an exercise in conceptual clarification:

The concept of social structure is widely used in sociology, often broadly, and with a variety of meanings. ... A generic difference is whether social structure is conceived explicitly as being composed of different elements and their interrelations or abstractly as a theoretical construct or model....

In his defence of the first sense, Blau also deliberately set a direction for the discipline. Taking 'social structure' to refer to 'the differentiated interrelated parts in a collectivity', he introduced 'structural parameter' as a means of treating the effects of differentiation, and devoted most of the address to its explication. Written in standard sociological terms, the address is rhetorically flat.² There are points to be made and elaborated, and Blau makes and elaborates them. When a trope is used, Blau informs the reader that a trope is being used.³ In general, he could be said to illustrate Gusfield's (1976: 17) *mot*, that "[t]he style of non-style is itself the style of science". If the oral version of the address resembled the published, it is difficult not to feel sympathy for an after-dinner audience sitting through its almost 11,000 words.

To be sure, the address had its flourishes, even if these refer to another face of the deliberative. Sociology was not only required to be theoretically clear but should be useful as well. Recalling a theme of his earlier work - "[the] possibility that free men become mere cogs in the bureaucratic machinery ... is one of the greatest threats to our liberty" (Blau and Scott 1963: xiii) - Blau's peroration was a call to action:

This is the challenge of the century, to find ways to curb the power of organizations in the face of their powerful opposition, without destroying in the process the organizations or democracy itself. Unless we can meet this challenge, the growing consolidation effected by organizations is likely to replace democratically instituted recurrent social change with alternate periods of social stagnation and revolutionary upheaval.

The threat is serious, and the time is late. Let us remember that we are within a brief decade of 1984. And let us endeavor to prove Orwell a false prophet.

² Indeed, Calhoun (1992: 137) described Blau as "perhaps *the* paradigmatic representative of 'standard American sociology' over the last forty years".

³ In adapting Granovetter's (1973) 'strength of weak ties', for example, Blau said, '[t]o use an analogy, a Gothic structure supported by multiple counter-balancing buttresses has replaced a Norman structure with a uniform solid foundation'.

But if the future threatened, the present could be celebrated. That is exemplified in Parsons' address.

Parsons as epideictic

Parsons too set his tone in his introduction. His work was being acknowledged, and he was not reluctant to insist on its importance:

It ... seems appropriate to take advantage of the present occasion to speak of the future prospects of that aspect of sociological science on which more than any other I feel qualified to speak.

The history of science testifies eloquently to the fundamental importance of the state of its theory to any scientific field. Theory is only one of several ingredients which must go into the total brew, but for progress beyond certain levels it is an indispensable one. ... I hope I may presume that my own election to its presidency by the membership of this society may be interpreted as an act of recognition of this importance of theory, and a vote of confidence in its future development.

Framed as a response to personal honour, Parsons' address was then an epideictic reaffirmation of the collectivity.

The celebratory linking of the personal and the disciplinary recurs throughout the address. Parsons' use of the image of sociology as a 'young science' is typical. When he told of 'an important personal experience which produced what I hope will prove to be a significant theoretical advance precisely in the field of general theory', he also insisted on the relation of this theoretical work to empirical research:

If I correctly assess the recipe for a really good brew of social science it is *absolutely imperative* that these two basic ingredients should get together and blend with each other. I do not think it fair to say that we are still in the stage of proto-science. But we are unquestionably in that of a distinctly *immature* science. If it is really to grow up and not regress into either of the two futilities of empiricist sterility or empirically irrelevant speculation, the synthesis must take place...

Epideictic fusion of the personal and the collective entails a difficult balance, and beyond his stress on research Parsons maintained it by various means. One was self-mockery. In both his introduction and conclusion, for example, he repeated the image of 'young science' with reference to a game played by his children: they would march about the house shouting 'The sociology is about to begin, said the man with the loudspeaker'. As judges of the field as a whole, he thought 'they were a bit on the conservative side', for after the 'proto-sociology' written before the generation of Durkheim, Weber, Pareto and Marshall, 'the sociology ... has been gathering force for a

generation and is now really under way'.⁴ Use of the tradition also allowed a collective appeal; whereas the work he was writing concurrently, *The Social System* (Parsons 1951) was to draw the scandalised charge that he had neglected the founders of American sociology (Faris 1953: 103), he referred in the address to Cooley, Sumner, Mead, Thomas and Park. Personal celebration was also collective commemoration.

Now although Aristotle had identified the forensic, the epideictic and the deliberative genres with the past, present and future respectively, it is evident that Parsons' response to the honour of the moment also entailed a looking back and a looking forward. The generality of that and other moves limits the analytical usefulness of the classical genres. But if they are treated as fractal rather than as mutually exclusive then the performativities given in each can still be evoked.

The genres as fractal

While Coser's, Blau's and Parsons' addresses have been taken as ideal-typical they cannot be so sharply separated. Common occasioning ensures common elements, and at different scales: at a subdisciplinary level, each is linked to the sociologies of knowledge; on a disciplinary scale, the genre in each address is interfused with the other two; and beyond the discipline, each entails a stress on national location. To accommodate those effects, the genres within presidential address are more usefully seen as fractal than as distinct.

It is not surprising that each president drew on the sociologies of knowledge, given that Merton had pioneered them in 'the discipline' during the period of his work with Parsons at Harvard, and that he had taught Coser and Blau at Columbia. References to him within the three addresses evoked different aspects of his work. In his one direct citation, Coser contrasted the concentration of path-analysts on 'making it' with the potential for class-analysis 'so brilliantly opened up' by Merton's 'seminal anomie paradigm'.⁵ But he also used Crane's Mertonian-Kuhnian synthesis, and, in granting that ethnomethodology *may* result in important findings, repeated the Mertonian orthodoxy that it is 'axiomatic among sociologists of knowledge that the origin of ideas does not prejudice their validity'. Blau likewise referred to Merton's

⁴ The occasioned modesty of this usage is evident in contrast with Parsons' (1959: 558) claim a decade later, in his paper on sociology as a profession, that it was only "within the last decade or so" that sociology had reached a first level of maturity as a scientific discipline.

⁵ Of course, Merton's (1938b) model of anomie was specifically structured around 'making it'.

more general work (on the meaning of 'group'), and also illustrated the integrative effect of relations between multiply defined 'insiders' and 'outsiders' through one of his papers in the sociology of sociology (Merton 1972). Parsons was most elaborate, with his address being one moment in a decades-long exchange over 'general' and 'middle range' theory. What Merton (1968b) was to describe as the codifying functions of 'paradigms' in middle range, Parsons accommodated in his version of 'theory':

general theory can provide a broadly orienting framework. It can also help to provide a common language to facilitate communication between workers in different branches of the field. It can serve to codify, interrelate and make available a vast amount of existing empirical knowledge. It also serves to call attention to gaps in our knowledge, and to provide canons for the criticism of theories and empirical generalizations. Finally, even if they cannot be systematically derived, it is indispensable to the systematic clarification of problems and the fruitful formulation of hypotheses.

From Parsons' overlapping with Merton's pre-Kuhnian sense of 'paradigm', through Coser's use of a Mertonian-Kuhnian model, to Blau's more general reference to 'insiders and outsiders', the three addresses collectively illustrate the common-sense sociology of sociological knowledge to be found in the full set.

They are as interfused at a disciplinary scale as they are with subdisciplinary taken-for-grantedness, for although Coser's address was read as forensic, Blau's as deliberative and Parsons' as epideictic, each is situationally given as implying all three rhetorical genres. Since Coser's theme of 'the uses of conflict' recalled one of the texts on which his reputation rested - his analysis of 'the functions of social conflict' (Coser 1956) - his description of his address as an 'exercise in the uses of controversy' situated it epideictically, while of course his call for sociologists to accept the 'major task' of analysing the differential effects of social structures was deliberative. Blau likewise linked his topic forensically to the tradition:

the study of the various forms of differentiation among people, their interrelations, the conditions producing them, and their implications is the distinctive task of sociology. No other discipline undertakes this important task, and sociologists too have neglected it, despite the theoretical emphasis on differentiation as a core sociological concept ever since Spencer.

Similarly, given his earlier work, his attention to 'organizations' and 'bureaucracy' was as epideictic as was Coser's to 'conflict'. This blurring is most evident in Parsons' address. Reiteration there of his thesis of a convergence between Weber, Durkheim, Marshall, Pareto and Freud as the basis for disciplinary theory fused the epideictic and forensic, while his peroration suggested all three genres:

It is my judgment that a great opportunity exists. ... Can American sociology seize this

opportunity? One of our greatest national resources is the capacity to rise to a great challenge once it is put before us.

We can do it if we put together the right *combination* of ingredients of the brew. Americans as scientists generally have been exceptionally strong on experimental work and empirical research. I have no doubt whatever of the capacity of American sociologists in this respect. But as *theorists* Americans have, relative to Europeans, not been so strong - hence the *special* challenge of the theoretical development of our field which justifies the theme of this address. If we American sociologists can rise to this part of the challenge the job will really get done. We are not in the habit of listening too carefully to the timid souls who say, why try it, it can't be done...

Past, present and future are as interwoven here as are the policing, celebratory and exemplary moments of the presidency.

Finally, Parsons' national location was repeated in the other two addresses. Since Coser had described the American authors and editors of path-analytic texts published under the auspices of the ASA as 'fixated on the problems of making it', his referent was clear when he spoke of 'the social structures in which we are enmeshed and which largely condition the course of our lives'. Blau concluded from the integrative effects of differentiation that 'a threat of growing consolidation exists in our society and other industrial ones'. It was posed by:

the dominant position of powerful organizations in contemporary society, such as the Pentagon, the White House, and huge conglomerates. ... That some other societies are still more centralized than ours is small consolation if we are, as we seem to be, on a rapid course to join them.

Sociology might even be identified with America. With a rather less suspicious view of 'organization', that was Parsons' claim:

Like all branches of American culture, the roots of sociology as a science are deep in Europe. Yet I like to think of sociology as in some sense peculiarly an American discipline, or at least an American opportunity. There is no doubt that we have the leadership now ... We certainly have all the makings for developing the technical know-how of research. We are good at organization which is coming to play an increasingly indispensable part in research ...

But however explicitly the point is made, it is implicit in the instability of referent of the first person plural in all the extracts quoted throughout this comparison. Given the difficulty of deciding whether 'we' and 'us' and 'our' refer to 'sociologists' or 'Americans', the collectivity of the discipline merges with its national milieu.

Typologically distinct, the three rhetorical genres are no more separable in practice than are the present, past and future to which, respectively, they are said to be directed. Any occasioned speech such as a presidential address reaffirms the collectivity of the moment by a diagnostic looking back and by a prescriptive or

exemplary looking forward. Each of the epideictic, the forensic and the deliberative moments is fractally active in each of the other two. Other fractal aspects of the addresses appear in their locating and their analytical framing, with the collectivity evoked in them extending well beyond the discipline, and with the common sense infusing them being both sub-and metadisciplinary: as suggested by the instability of referent of the first person plural, the policing, celebrating, and prescribing of American sociology may be applied as much to America at large; as suggested by the overt and implicit uses of the sociologies of knowledge, sociology is in part its own effect. The discipline of the here and now is defined by a simultaneous looking forward looking back, by a looking out and a looking in.

So as seen in Coser's, Blau's and Parsons' addresses, the classical division of rhetoric into forensic, deliberative and epideictic genres does offer some purchase but cannot stand alone. Since each address met the requirements of each genre, the trans-scalar interpenetration of fractal ordering is preferable to the preemptions of typological analysis. But if analysis is limited, it is not precluded. As Mannheim said, the best feature of positivism was its metaphysics, and that too is included in fractal ordering. It is elaborated in the following chapter.

CHAPTER 6 : THE PRESIDENTS AND PRESIDENTIAL ADDRESSES - REAL AND IDEAL FACTORS

This chapter consists of an analytical extension of the reading of American sociology, in which 'the discipline' was shown as a fractal ordering, and presidential addresses to the ASS/A as documentary of it. Following the defining imaginary of the classical sociology of knowledge - discovery of "the orderly sequence of the *reciprocal effects* of ideal *and* real factors" (Scheler 1925: 36) - 'the discipline' as base or civilisational context, the source of 'real factors', will be related quantitatively to the 'ideal factors' of the addresses as superstructural or cultural artefacts. If it ends in the jaggedness of fractal ordering rather than in an 'orderly sequence', the interaction of real and ideal factors found here still supports both the modelling of 'discipline' and the reading of 'the discipline'.

Since this appeal to positivist metaphysics derives from the same textual ethnography as in the previous chapter, and since this requires that Scheler's binary ontologies of 'world' and 'spirit' be collapsed, use of his distinction between 'real' and 'ideal' is metaphorical. While the presidents' organisational characteristics will be taken as real and features of their addresses as ideal, both sets of factors are equally constitutive of disciplinary networks. Their separation is no more than a convenience, and is possible only within severe constraints. Due to the reading of 'the discipline' adopted as a benchmark, to the generic specificity of the addresses, and to the methods used for each set of factors, no definite matching of them is either expected or found.

The 'congenitally failing' character of 'discipline' was confirmed in accounts of 'the discipline'. American sociology was shown as an always contingent effect of the 'collectivity', 'science' and 'usefulness' deriving from variously religious and secular sources and subject to variously centripetal and centrifugal strains. While the 'golden age' of Parsonian/Mertonian structural functionalism lasted through the 1950s and into the 1960s, 'the discipline' is commonly depicted as having been in crisis ever since. That is an uncertain benchmark to use for analysis of it.

The genre of 'presidential address' is just as unstable. Since the addresses were shown to be occasioned as celebrations of personal achievement, as exemplary sociology, and as renewals of collectivity, the trace of each generic moment might be expected in

compositional features - ideal factors - amenable to linking with the real factors of the presidents' disciplinary characteristics. But since those generic moments are fractally trinitarian rather than analytically separable, they too rest on shifting ground.

Finally, quantitative methods for the relating of real and ideal factors are all rendered uncertain through interpretive foreclosures. Operationalisation of real and ideal factors into well-defined independent and dependent variables is impossible. Any attempt repeats the preemptions found in the civilisational/cultural circularities of the sociologies of knowledge.

But if all that is hardly a solid basis for analysis, these methods can still be used. Uncertainties in disciplinary context, in the genre of the addresses, and in the operationalisation of real and ideal factors, are effects of the contingency implied in 'discipline' as failing and subsumed in fractal ordering. Scheler's 'orderly sequence' might be unattainable, but as long as the self-limiting given in the image is sustained, then the presidents' organisational characteristics can be linked with features of their addresses on the grounds which Law (1994: 107, emphasis removed) claimed for his 'modes of ordering':

it is plausible to go out and look for fairly coherent and large scale ordering patterns in the networks of the social. It is ... plausible to look for orderings which (to the extent they are performed) generate, define, and interrelate elements in relatively coherent ways.

A relatively coherent patterning of the presidents' 'real' and 'ideal' factors will be developed in this chapter, in three phases.

In the first, the presidents' real factors will be derived from a version of age-, period- and cohort-analysis, from their educational, honorific, and organisational careers, and from their gender, and fairly coherent orderings will be shown in them. In a circular test of the factors against the context and of the context against the factors, these will be seen as roughly consistent with accounts of 'the discipline', and in a way which disrupts the distinctions of civilisation from culture and of the epistemological from the political.

Secondly, ideal factors in presidential addresses will be derived from content- and citation-analyses. Fairly regular patterns will be seen here as well. The terms and repertoires examined in the content-analysis show that the addresses are generically distinct while being identifiably sociological, that 'discipline' is an issue in them, and that they may be treated as sociologies of sociological knowledge. The citation-analysis likewise suggests large-scale trends. Again, both sets of patternings are

roughly consistent with 'the discipline', and with the same diremptive effect.

While any attempt at statistical correlation between real and ideal factors is precluded by the difficulties of their operationalisation, an inductive ordering is still possible. So, thirdly, the results of all the analyses will be collected in a 'disciplinary matrix' and reconciled through a method appropriate for heterogeneous and large-scale data-sets, correspondence-analysis. Once again, a fairly coherent patterning will be shown. Since it is roughly consistent with the model of disciplinarity, disciplining and discipline, and since that has been found to be consistent with 'the discipline', a degree of quantitative support will have been found for fractal ordering: the relation between 'the discipline' and disciplinary artefacts is real like nature. But at the same time, the jaggedness of the regularity found will be taken as a warrant for the rhetorical reading of the addresses in Part III of the thesis.

THE PRESIDENTS - REAL FACTORS

Since the presidents are formal representatives of 'the discipline', patterns in their disciplinary attributes should be documentary of it. Specifically, orderings are expected to reflect, unevenly, the trends of consolidation and dispersion which recur in accounts of American sociology. While any number of the presidents' characteristics could be taken as symptomatic - such as regional and religious origins or academic affiliation at time of election (cf. Odum 1951; Wolfe 1992) - attention here is limited to five which do not require first-hand knowledge, and which are largely available from the ASS/A's published records: the presidents' ages; the sources of their doctorates; the extent of their organisational engagement; their election or otherwise to the vice-presidency; and their gender. After each has been patterned, those factors will be jointly compared with the reading of 'the discipline'. The overall fit is good, so long as the distinction between civilisation and culture is collapsed.

Age and Cohort

'Age' has long been treated as an element of social structure, and that same effect should be expected in the discipline. Just as Lipset (1960), say, sought a link between ageing and conservatism, so older presidents might espouse a more conservative view of the discipline. But since the already intractable problems in the study of age are further

complicated by the smallness of the sample and by the difficulty of defining the presidents' generational 'entelechies', analysis here is kept very basic. In a move which serves throughout this chapter, the presidents are divided into six decennial cohorts, with no attempt made to find either period effects or definitive events beyond their election. Then, it is simply hypothesised that election at a relatively early age indexes recognition of work at the cutting edge of orthodoxy, and that, conversely, late election indicates the belated recognition seen in the discussion of Sorokin. To allow for any effects of late entry to the discipline, the presidents' 'actual ages' are separated from their 'disciplinary ages', with the latter defined as the difference between year of doctorate and year of presidency.¹

Location of three dates for all presidents - year of birth, year of doctorate, and year of presidency - allowed calculation of the two ages. Two cases are anomalous. As neither Homans {1964} nor Riley {1986} held a doctorate, they were given notional PhDs on the basis of professional affiliation and contribution: Homans is deemed to have earned his in 1939, the year he completed his Junior Fellowship at Harvard; and Riley, who earned an MA in 1937, who worked at Harvard in the late 1930s, and who was established enough by 1949 to be secretary/treasurer of the ASS and managing editor of the *ASR*, to have graduated in 1940. The full set of data is in Table B.1 in Appendix B, and the decennial mean ages taken from it are shown here in Table 6.1.

Table 6.1: Mean disciplinary (D) and actual (A) ages of presidents over six decennia

Period	D	A
1935-44	24.2	54.9
1945-54	25.1	54.6
1955-64	27.5	55.3
1965-74	29.0	59.3
1975-84	31.6	62.1
1985-94	32.9	62.3
Overall means	28.4	58.1

The pattern of ragged increase on both measures is consistent with the post-war growth in membership of the ASS/A; with greater numbers of potential honorands, those eventually elected simply had to wait longer. To smooth out that effect in testing for the relation of 'age' and 'orthodoxy', disciplinary and actual ages were standardised by

¹ cf. Crane's (1972) and Cole's (1979) 'professional age'.

the decennial means. Presidents with the highest and lowest scores on each age are shown in Tables 6.2 and 6.3.

Table 6.2: Presidents with highest (DH, descending order) and lowest (DL, ascending order) standardised disciplinary ages.

DH	DL
1. Znaniecki {1954}	1. Williams {1958}
2. Sorokin {1965}	2. Vance {1944}
3. MacIver {1940}	3. Coser {1975}
4. Riley {1986}	4. Huber {1989}
5. Lee {1976}	5. Frazier {1948}
6. Lipset {1993}	5. Cottrell {1950}
7. Lazarsfeld {1962}	7. Erikson {1985}
7. Hughes {1963}	8. Turner {1969}
9. Hankins {1938}	9. Lundberg {1943}
10. Whyte {1981}	10. Blau {1974}

Table 6.3: Presidents with highest (AH, descending order) and lowest (AL, ascending order) standardised actual ages.

AH	AL
1. Znaniecki {1954}	1. Williams {1958}
2. Sorokin {1965}	2. Vance {1944}
3. Riley {1986}	3. Turner {1969}
4. Hughes {1963}	4. Merton {1957}
5. Sanderson {1942}	5. Blalock {1979}
6. E. Faris {1937}	6. Chapin {1935}
7. Lipset {1993}	7. Parsons {1949}
8. Taylor {1946}	8. Erikson {1985}
9. Komarovsky {1973}	9. Lundberg {1943}
10. Lee {1976}	10. Moore {1966}

The hypothesis that early election indexes centrality of the presidents' work is only raggedly supported. Since Parsons {1949} and Merton {1957} themselves, and several of their students - Williams {1958} and Moore {1966}, Blau {1974} and Coser {1975} - are among those with the lowest ages, the linking of the golden age to their synthesis does appear justified. But since others of their students and colleagues, like Lazarsfeld {1962}, Komarovsky {1973}, Riley {1986} or Lipset {1993}, are among the presidents with the highest ages, and since a range of alternative sociologies are indexed by early election, the expectation of unevenness is also confirmed.

The 'disciplinary recognition' of leading work might be supplemented by a

dynamic of seasonableness or 'disciplinary justice'.² The mounting sense of obligation seen in debates over Sorokin's election (Johnston 1987) is also plausible for presidents such as Riley (1986) and Lipset (1993) - victims of a reaction against Harvard/Columbia ? - or Znaniecki (1954), who, after his early collaboration with W. I. Thomas, spent much of his career outside the United States. Honour would then be as much an effect of a moral community as of judgements over a disciplinary mainstream; it would be cultural as much as civilisational.

A similar uncertainty ensues from a patterning of the schools at which the presidents obtained their doctorates.

School

Three universities are especially notable in accounts of American sociology: Chicago was organisationally central until 1935; Harvard was Parsons' base; and after early prominence under Giddings, Columbia again became important through Merton and Lazarsfeld. That pattern should be expected in the schools from which the presidents obtained their doctorates, even if, again, the pattern is expected to be uneven.

As with 'age', the breakdown here is kept basic, with no attempt made to trace specific relationships between teachers and students.³ The sources of all presidents' doctorates (with Homans and Riley again taken as having graduated from Harvard) are listed in Table B.2 in Appendix B, and, as shown in Table 6.4, classified into 'Chicago', 'Columbia', 'Harvard', 'foreign universities', and American universities and colleges other than the leading three.

² Such a dynamic is consistent with Merton's (1984: 265-6) Durkheimian 'socially expected durations': these are "socially prescribed or collectively patterned expectations about temporal durations imbedded in social structures of various kinds".

³ Mullins (1973) set out many of these links. It might be noted that 41 of the 60 presidents appeared in his lists of 'important' sociologists: 28 in 'Standard American sociology', 9 in 'Symbolic Interactionism', 3 in only 'New Causal Theory', and 1 in only 'Small Group Theory'.

Table 6.4: Sources of presidents' doctorates, over six decennia

Period	Chicago	Columbia	Harvard	Foreign	Other US
1935-44	4	2	-	1	3
1945-54	4	-	-	3	3
1955-64	4	-	4	1	1
1965-74	3	2	2	1	2
1975-84	3	3	-	-	4
1985-94	2	2	1	-	5
Totals	20	9	7	6	18

The expected pattern is found. Given that four of the presidents with foreign doctorates taught at Harvard or Columbia - Sorokin {1965} and Parsons {1949}, MacIver {1940} and Lazarsfeld {1962} - the stepped cohort-effect in the middle three columns is especially striking. That clustering accounts for slightly over one-third of the presidents. Another third held doctorates from Chicago, and since several of these - Frazier {1948}, Cottrell {1950}, Turner {1969} and Erikson {1985} - appeared among those with the lowest ages, the continuing salience of Chicago is evident. To be sure, Chicago can neither be easily separated from Harvard/Columbia, given cross-hiring, nor neatly summarised (Faris 1970; Bulmer 1984; Harvey 1987; Camic 1995). But even if it is difficult to impute unity to a group including Sanderson {1942} and Cottrell {1950}, Stouffer {1953} and Blumer {1956}, or Hauser {1968} and Goffman {1982}, the persistence of 'Chicago sociology' is notable. Roughly another third of the presidents held doctorates from other American universities, and again, several of these were listed among those with the lowest ages: Lundberg {1943}, Vance {1944}, Blalock {1979}, and Huber {1989}.

So while the cohesion of the golden age, and the place of Harvard/Columbia in it, have again been confirmed, so too have been both an alternative tradition and a dispersive context. A similar sense of provisional coherence is found in the patterning of the presidents' 'organisational embeddedness'.

Organisational Embeddedness

The uncertainty of the relation between 'merit' and 'success' in the ASS/A was stressed in the previous chapter. In an ordering of the presidents' backgrounds which amounts to a rough test of the hypotheses that honour within the discipline is an effect of involvement in its formal organisation, the presidents' 'embeddedness' was gauged, from their service as vice-president, as a member of council, or as secretary. It was

expected, first, that 'embeddedness' would be highest during the golden age, and secondly, that a rough distinction between 'merit' and 'success' would be evident.

Given the constitutional changes in the ASS/A, a number of simplifications were made to ensure uniformity over the sixty-year period of the study. Service on the council as 'president-elect' was ignored. The 'first-vice-presidency' of the earlier years was equated with the 'vice-presidency' of the later, but, as the outcome of a separate election, the 'second-vice-presidency' was not assimilated to the 'vice-presidency-elect', being counted instead as membership of council. As for the council, representation from regional and special-interest societies was ignored; only election by the members at large was counted. No allowance was made for the change of the secretaryship to an elective office, but Wirth's and Blumer's services as secretary before 1936 have been excluded, since they were appointed from within the Department at Chicago. In an adaptation of Straus and Radel's (1969: 1) coding of the offices, the vice-presidency was assigned a value of 3, the secretaryship 2 for each year of office, and membership of council 1 for each year. Each president's total score is his/her 'embeddedness'. The full results, shown in Table B.3 in Appendix B, are summarised here in Table 6.5.

Table 6.5: Presidents' mean embeddedness (Em), over six decennia

Period	Em
1935-1944	4.4
1945-1954	6.2
1955-1964	6.6
1965-1974	5.0
1975-1984	4.2
1985-1994	3.9

The first hypothesis is supported, since the rise and fall of embeddedness over the six decennia is expected from accounts of consolidation and then fragmentation within American sociology. But the second is more problematic, as seen when the five presidents with the highest scores are matched against those who scored zero. This comparison is shown in Table 6.6.

Table 6.6: Presidents with highest (EmH, descending order) and zero (EmZ) embeddedness

EmH	EmZ
1. Williams {1958}	Lazarsfeld {1962}
2. Thomas {1952}	Sorokin {1965}
2. D. Young {1955}	Lee {1976}
4. K. Young {1945}	Goffman {1982}
4. Parsons {1949}	Wilson {1990}

It must be said first that this table shows the limits of the organisational analysis. Since Parsons' score derived entirely from his five years as secretary, an office he held well after his presidency, since Williams' was increased for the same reason, and since Kimball Young served one term on council after his presidency, their embeddedness appears more an *effect* of honour than the converse. Lee's presence in the other column is just as misleading, since he had in fact served for years on the council as the SSSP's representative; other presidents' scores likewise do not reflect their actual engagement. But even given those difficulties, and the limits of the coding across organisational changes, results are still suggestive. Since Thomas', Donald Young's, and Kimball Young's work is now little used, organisational engagement appears tenuously linked to 'merit'.⁴ In the other column, Sorokin's work is still disputed, and it is too early to judge the durability of Wilson's, but there is no doubt over Lazarsfeld's or Goffman's disciplinary eminence. So 'success' and 'merit' do appear to be separate effects.

The contingency of organisational embeddedness is more evident when one component of it, the vice-presidency, is treated separately.

Vice-Presidency

Whether or not embeddedness indexed 'success', it was expected that election to the council, to the vice-presidency, and to the presidency would amount to something of an honorific trajectory through the ASS/A; that is part of the 'reward system' of sociology. As coding proceeded, it became clear that this was misplaced. Or rather, it was evident that a more-or-less established structure has undergone a disruption which might be taken as further confirmation of fragmentation in 'the discipline'; the link between the presidency and the vice-presidency has become far looser than it once was.

⁴ Thomas, Donald Young, and Kimball Young ranked 43, 51 and 39 respectively among the 63 presidents in Westie's (1973) test of 'disciplinary survival'. Of the three, only Kimball Young appeared in Mullins' (1973) lists of 'important' sociologists.

The data for this comparison are in Appendix B. Table B.4 lists all vice-presidents since 1935, with those who subsequently became president starred, and in Table B.5 the order is reversed; the presidents are listed, with former vice-presidents starred. Results are summarised in Table 6.7.

Table 6.7: Decennial numbers of vice-presidents who became president (nV), and of presidents who had been vice-president (nP).

Period	nV	nP
1935-1944	4	6
1945-1954	7	6
1955-1964	8	6
1965-1974	5	7
1975-1984	3	1
1985-1994	1	2

Since any number of former vice-presidents may yet be president, figures for the last two or three decennia in the first column are provisional; Smelser, for example, president in 1997, was vice-president in 1975, while Quadagno, to be president in 1998, held the junior office in 1993. Even so, a discontinuity appears at the cusp of the fourth and fifth decennia. Of the first 40 presidents, a majority (25) had previously been vice-president, with an average period of about five years between their elections to the two offices, while only three of the remaining 20 had held the junior office. What had been at least a rough trajectory was disrupted.

The earlier suggestion that the increasing disciplinary and actual ages of the presidents was an effect of increasing membership in the ASS/A can be repeated here: a bifurcated honorific structure allowed a greater number of sociologists to be recognised. But this accommodation of more honorands also reflects substantive dispersion, for while those following the near-standard trajectory might be seen as representing the disciplinary mainstream in the first four decennia, such an attribution is less plausible in the last two. A greater variety of sociologies was recognised. Since the presidents' formal authority had, if anything, been weakened in the constitutional revision of 1967, and that of the vice-presidents increased (Moore 1965/6), this recognition of pluralism was an organisational reflection of substantive practice.

It is more complicated still. The honorific structure appears to be gendered.

Gender

Since the discontinuity at the cusp of the fourth and fifth decennia coincided with a heightened prominence of women in the ASA, 'gender' must be included in the dynamics of 'success' and 'merit'. This is another source of simultaneous consolidation and dispersion within 'the discipline'. The shift is evident in the increasing numbers of women elected to the presidency and the vice-presidency, as shown in Table 6.8.

Table 6.8: Numbers of male and female presidents and vice-presidents, over six decennia.

Period	Presidents		Vice-presidents	
	M	F	M	F
1935-1944	10	-	9	1 ⁵
1945-1954	9	1	7	3 ⁵
1955-1964	10	-	10	-
1965-1974	9	1	8	2
1975-1984	9	1	5	5
1985-1994	8	2	4	6
Totals	55	5	43	17

The imbrication of 'gender' in the honorific trajectory is evident in that only three presidents in the last two decennia had been vice-president, that only three presidents in that period were women, and that the two sets of three coincide: Alice Rossi {1978, 1983}, Riley {1974, 1986}, and Huber {1982, 1989}. If the anomalous Turner is ignored, then Smelser {1975, 1997} is the first male vice-president to have been elevated since Goode {1968, 1972}. At least part of that trend continues: of the office-holders elected since Gamson and Thorne for 1994, two of the four presidents/presidents-elect are women, as are three of the vice-presidents/vice-presidents-elect.

This trend might index another form of the 'disciplinary justice' proposed under 'age', and with it another set of messy interactions between 'civilisation' and 'culture'. Amidst a more diffuse acknowledgement of imbalance, the greater prominence of women is an effect of organisational reform (Simpson and Simpson 1994) and of networked lobbying (D'Antonio and Tuch 1991), and what it means for sociology remains contested. On the one hand, a 'missing feminist revolution in sociology' has been diagnosed (Stacey and Thorne 1985; Ferree and Hall 1996); on the other, it has been suggested that the discipline is becoming marginalised through a focus on women's issues (Iaquinta 1991:

⁵ Dorothy Thomas, the only vice-president elected twice, held the office in 1939 and 1949.

38).⁶ As expected, and as in each of the other real factors, 'gender' then suggests more than can be straightforwardly resolved.

The Presidents - Real Factors: Reprise

The orderings of the presidents' characteristics - their real factors - presented throughout this section are all consistent with depictions of American sociology as having been consolidated, from about 1950 to 1970, around Parsonian/Mertonian structural-functionalism, but as undergoing a crisis of fragmentation since then. Parsons and Merton themselves and presidents working in their terms were among those with the lowest disciplinary and/or actual ages. Graduates from Harvard and Columbia were elected at the expected times. The presidents' organisational embeddedness rose and fell as anticipated. The ASA's honorific structure bifurcated at about the time when fragmentation is said to have begun. The increasing prominence of women was a form of simultaneous inclusion and disruption. That match with conventional accounts gives a certain validity to the orderings here.

But at the same time, consistency was always jagged. The patterns found had no more than the 'relative coherence' expected from fractal ordering. Indeed, given the fractal modelling of 'discipline' as failing, discovery of close agreements would have been surprising. The same is true of the methods used. As seen with embeddedness, coding might gloss any number of anomalies, while since the meanings attributable to age, education, organisation, honour or gender are always already uncertain, anything more than ragged consistency would not be possible.

So success in these orderings of 'real factors' is being claimed for both their fit and their lack of fit with the orthodox account. But since the claim of success is limited by the same logic of fractal ordering which results in it, that is just a version of the circularity of the sociologies of knowledge. Once the distinction between civilisation and culture is lifted, analytical separations are impossible. As seen in the dynamic of 'disciplinary justice' or in the uncertainty over 'success' and 'merit', sociological analysis is inseparable from the workings of sociology as a moral community.

That same interfusion of formal and informal should be expected in analysis of the 'ideal factors' of the presidential addresses.

⁶ It should be stressed that Iaquineta, in his presidential address to the Wisconsin Sociological Association, was reporting rather than supporting that view.

PRESIDENTIAL ADDRESSES - IDEAL FACTORS

Where 'real factors' denoted the presidents' organisational attributes, 'ideal factors' refer to features of their addresses. Since the addresses are documentary of 'the discipline', those features are expected first, to be patternable along the same lines as in the previous section, and secondly, to be linkable with the orderings there. On that basis, the ideal factors of the addresses are ordered by content- and citation-analyses. 'Fairly regular' patterns are found in each, and if these are not quite Scheler's 'orderly sequence', they are as consistent with fractal ordering as were those of the real factors.

Content-Analysis

As a method for the study of sets of texts across time, in which shifts in the frequencies of key terms or expressions are used to index shifts in context, content-analysis is well-suited to study of regularly published disciplinary documents. In this case, it is expected that the waxing and waning of the terms and repertoires used in the addresses will reflect the processes given in accounts of 'the discipline'. But it is expected, too, that the matching of content and context will only ever be 'fairly coherent'.

The terms and repertoires to be counted are derived from the proposal that as effects of disciplinary honour, disciplinary exemplification, and disciplinary policing, presidential addresses constitute a generically distinct folk sociology of sociological knowledge. That in turn followed from the conclusion in Part I of the thesis, that as representations of 'discipline', sociologies of knowledge are both problematisations and enactments of 'the modern'. So the terms to be analysed are taken from the fractal ordering of the disciplinary into moments of disciplinarity, disciplining and discipline, and of disciplinarity into moments of collectivity, science, and usefulness. It will be shown through their relative frequencies in the addresses that:

- The use of the first person, singular and plural, marks the addresses as expressions of personal honour and collective renewal, and that these effects are generically distinctive;
- The use of 'sociology' itself and of a 'vocabulary of sociology' gives the addresses a specifically disciplinary location;
- As well as the collectivity already suggested, the scientific and useful moments of disciplinarity are recurrently invoked;
- Use of a 'normative repertoire' throughout the addresses confirms their 'disciplining' moment;

- Use of a 'locational repertoire' places the addresses as disciplinary in more general contexts and processes.
- The uses of 'knowledge', 'discipline', 'represent', and 'modern' set the required link between the addresses and the formal sociologies of knowledge.

Within the limits of fractal ordering, that set of findings amounts to a quantitative justification for treatment of the addresses as folk sociologies of sociological knowledge.

Now, it was stressed in Chapter 4 that the use of content-analysis requires what might be seen as either crudity or austerity. In principle, the problem arising from the polysemic, intertextual, and thus always already both epistemological and political character of sociological terms can be resolved by treatment of them as emergent markers rather than as bearers of fixed meanings. In practice, the lifting of preemptive distinctions between the civilisational and the cultural requires suspension of *a priori* decisions as to whether or not a term is being used in its technical sense. On the assumption of polysemic effectivity, all instances of a given term must be counted, regardless of context, and if that results in a frequency being at best a crude indicator, it also gives it a certain austere rigour.

On that basis, the same method was used for analysis of each repertoire and term. Unless otherwise specified, each occurrence of the word(s) being examined was noted in each address, and the overall frequency calculated. To enable comparison between the addresses, this frequency was then standardised as a rate per thousand words, and mean usage calculated for the six decennial cohorts. It is shown first that the presidents' use of the first person marks the addresses as generically distinct.

First Person Insofar as the addresses are both responses to personal honour and directed to the collectivity of sociologists, heavy use should be found, respectively, of the first person singular and the first person plural. A count was then made of all uses, other than in quotations, of 'I', 'me', 'myself' and 'my/mine', and of 'we', 'us', 'ourselves' and 'our(s)'. Only two presidents did not use the singular, and all used the plural. To check the generic distinctiveness of this combination, the addresses were compared with a matched sample of sociological papers, comprising the first single-authored standard article in each of the first sixty volumes of the *ASR*; 'standard' was taken to exclude the published versions of occasioned speeches. Results are shown in Table 6.9.

Table 6.9: Mean standardised use of first person singular (Fs) and plural (Fp), in presidential addresses and in standard articles.

Period	Addresses		Standard Articles	
	Fs	Fp	Fs	Fp
1935-1944	3.32	14.69	0.38	3.51
1945-1954	7.73	11.24	0.24	8.58
1955-1964	4.37	6.28	0.54	2.89
1965-1974	4.69	7.01	1.21	3.21
1975-1984	3.94	7.31	0.95	2.74
1985-1994	5.60	8.02	2.11	2.00
Overall means	4.94	9.09	0.91	3.82

The difference is clear. The patterns of rise and fall are dissimilar, use of the first person singular is over five times higher in the addresses, and that of the plural more than twice as high. By this measure alone, the addresses are distinct from standard sociology, having the epideictic fusion of the personal and the collective expected from their occasioning. But the language in them is still disciplinary language, as may be seen in the 'vocabulary of sociology'.

Vocabulary of Sociology The 'vocabulary of sociology' - a measure of the deliberative moment in the addresses - consists of the leading ten from a list of terms commonly used in American sociology, and chosen through an alerted observation of recurrence. These are: 'culture', 'function', 'group', 'process', 'role', 'social', 'society', 'structure', 'system' and 'values'. Derivatives of each were included, so that, say, 'culturally', 'societal' and 'functional' were counted with the respective core terms. With the exception of 'socialism' as a derivative of 'social', all occurrences were counted. No president used less than six of the terms, and twenty used all ten; modal usage was nine. Table 6.10 shows the decennial mean total use of the vocabulary, overall mean use of each word, and the number of using presidents.

Table 6.10: Mean use of the vocabulary of sociology across six decennia, with overall mean use of each term (Te) and number of using presidents (n)

Word	1935-44	1945-54	1955-64	1965-74	1975-84	1985-94	Te	n
Social	10.67	7.56	6.07	7.45	7.25	5.90	7.53	60
Society	2.46	2.38	3.36	2.72	1.66	2.82	2.57	55
Group	2.18	3.38	2.31	4.15	1.64	1.68	2.55	59
System	0.78	2.68	2.12	3.15	2.57	1.89	2.20	58
Culture	0.60	1.45	0.93	1.96	2.40	1.59	1.49	54
Structure	0.57	1.72	1.08	1.64	1.21	2.23	1.41	56
Process	1.37	1.23	1.66	1.07	1.29	1.56	1.36	58
Function	0.72	1.47	4.25	0.49	0.45	0.52	1.32	55
Values	1.52	1.28	0.90	1.29	1.37	0.73	1.18	57
Role	0.27	2.25	0.68	2.48	0.76	0.63	1.18	48
Totals	21.14	25.39	23.36	26.68	20.60	19.52		

It should be stressed that these results reflect contested usage. The score for 'function' in the third decennium, for example, arose from both Davis' {1959} defence and Homans' {1964} critique of functionalism. Or while the salience of 'role' in the second decennium was due to several presidents, the similar score in the fourth derived almost entirely from Komarovskiy's {1973} defence of the term against what she held to be excessive attack. But since there can be little doubt over the salience of the terms in American sociology, however they are contested, and since the ragged rise and fall seen in the six totals is the pattern familiar from the real factors, this repertoire is used as a standard of comparison throughout the rest of the content-analyses. It is seen next that beside using a vocabulary of sociology, the presidents were concerned *with* sociology.

Sociology A reading of the addresses as folk sociologies of sociological knowledge requires minimally that they be established as reflexive. Already apparent in the fact that 23 of the 60 presidents included 'sociology', 'sociologists', or 'sociological' in the titles of their addresses, this is confirmed more strongly in the textual use of 'sociology' and its derivatives. The results of this count are shown in Table 6.11.

Table 6.11: Mean standardised use of 'sociology' (So) over six decennia, with number of using presidents (n).

Period	So	n
1935-1944	5.63	10
1945-1954	4.54	10
1955-1964	5.38	10
1965-1974	3.28	10
1975-1984	3.72	9
1985-1994	4.58	10
Overall mean/total	4.52	59

Used by all presidents but Hawley [1978], having a mean score between the two leading terms in the vocabulary, and showing a ragged consistency over six decennia, 'sociology' itself is clearly a focal concern throughout the addresses. That reflexivity confirms the 'collectivity' explicit in use of the first person plural and implied in the vocabulary. The other two moments of disciplinarity, 'science' and 'usefulness', are also prominent.

Science A concern with the scientificity of sociology is indexed in two ways. First, all occurrences of 'science' itself and its derivatives were counted, and then, on Gilbert and Mulkay's (1984) example, 'empiricist' and 'interpretivist' repertoires were developed. As with the main vocabulary, these were assembled partly on *a priori* grounds and partly through observation of recurrence. The first comprised 'analysis', 'empirical', 'research', 'test' and 'variable'; and the second 'inquire', 'interpret', 'mean', 'study' and 'understand'. Each term as usual was taken to include all its derivatives, and with one set of exceptions all occurrences counted; uses of 'mean' in expressions such as 'means of production' or where it denoted 'average' were excluded. No president used less than two terms from either repertoire. For the empiricist, twenty used all five, which was also the modal use, and in the interpretivist, modal use was four, while fifteen presidents used all five. The full set of results is given in Table 6.12.

Table 6.12: Mean standardised use of 'science' (Sc), and of the empiricist (Emp) and interpretivist (Int) repertoires over six decennia, with numbers of using presidents (n).

Period	Sc	n	Emp	n	Int	n
1935-1944	4.86	9	2.90	10	3.66	10
1945-1954	2.63	10	7.19	10	5.91	10
1955-1964	3.81	10	10.10	10	5.39	10
1965-1974	2.33	8	5.25	10	4.86	10
1975-1984	3.72	10	8.04	10	4.02	10
1985-1994	1.04	10	6.62	10	5.46	10
Overall means/totals	2.92	57	6.68	60	4.88	60

Since each mean falls between those of the two leading terms in the vocabulary, the salience of these orderings is clear. Science may have been most invoked when 'the discipline' is commonly held to have been least scientific, and use of the empiricist repertoire must always be balanced against the interpretivist, but 'science' is an abiding concern. A similar case can be made for 'usefulness'.

Usefulness The ameliorative purposiveness of sociological knowledge is indexed by a 'repertoire of usefulness', derived through the usual combination of *a priori* expectation and observation of recurrence. All instances of the terms in it - 'apply', 'practice', 'problem', 'relevance', 'useful', and 'utility', together with their derivatives - were counted. No president used less than three, and sixteen used all six; modal use was five. Scores for the repertoire are shown in Table 6.13.

Table 6.13: Mean standardised use of the 'repertoire of usefulness' (U) over six decennia, with numbers of using presidents (n).

Period	U	n
1935-1944	3.79	10
1945-1954	5.50	10
1955-1964	5.13	10
1965-1974	2.63	10
1975-1984	5.62	10
1985-1994	3.71	10
Overall mean/total	4.40	60

Since once again the overall score falls between those of the two leading terms in the vocabulary, the presence of this imaginary of sociology is confirmed. So far, then, the

addresses have been shown to be of and about sociology, and to be generically distinct fusions of the epideictic and deliberative and of the three moments of disciplinarity: science, usefulness, and collectivity. It will now be shown that they are also infused with the forensic normativity required of 'disciplining'.

Normativity A presidential address entails occasioned policing of the discipline.

Given the presidents' occasioned constraints, it is expected that they will use normative language, but not heavily. To limit preemptions of the normative, the repertoire assembled to index this forensic strain consists only of 'must', 'ought' and 'should'. All occurrences were counted, regardless of referent. Since nine presidents used all three terms, and two used only one, modal use was two. Results are shown in Table 6.14.

Table 6.14: Mean standardised use of the normative repertoire (N) over six decennia, with numbers of using presidents (n).

Period	N	n
1935-1944	3.29	10
1945-1954	3.23	10
1955-1964	2.31	10
1965-1974	2.50	10
1975-1984	1.59	10
1985-1994	2.07	10
Overall mean/total	2.50	60

Since the mean is comparable with those of such leading terms in the vocabulary as 'society' (2.57, 55) and 'group' (2.55, 59), the addresses' forensic strain is relatively prominent, albeit less so than the moments of 'disciplinarity'. Now, it was found in the sociologies of knowledge that 'disciplining' extended beyond sociology proper. A discipline is a fractal of social orderings more generally, which are also policed. This process is evident in the addresses, for they are all located in the here and now.

Location Wilner's (1985) claim that American sociology was not 'relevant' to the events of the day was noted in Chapter 4, as were the difficulties of linking specific events and knowledge. But on a Mannheimian view of the documentary, the addresses cannot not be 'relevant'. That is established here through a 'locational repertoire'. While no attempt is made to specify the meanings of events, it can be shown that the presidents

locate themselves nationally and temporally, and thus appeal at least tacitly to their audiences' perceptions of more general orderings. National location is indexed through all uses of 'America' and of the various versions of 'United States'. Thirty-three presidents used both, while six used neither. 'Nowness' is gauged by 'contemporary', 'current', 'now' (used by all presidents), 'present', 'recent', and 'today'; one president used only 'now', fourteen used all 6, and modal use was four. Scores from all counts were combined in the 'locational repertoire' shown in Table 6.15

Table 6.15: Mean standardised use of the locational repertoire (L) over six decennia, with numbers of using presidents (n).

Period	L	n
1935-1944	4.01	10
1945-1954	3.93	10
1955-1964	4.58	10
1965-1974	4.29	10
1975-1984	2.62	10
1985-1994	4.84	10
Overall mean/total	4.04	60

Since this repertoire too is comparable with the leading terms in the vocabulary, the addresses can be taken as well grounded in their place and time. The disciplinarity of the addresses was found earlier, and now the pervasively grounded normativity of their disciplining moment has been shown as well.

Knowledge, Discipline, Represent, Modern Finally, since the addresses are being read as folk sociologies of knowledge, the 'knowledge', 'discipline' and 'represent' locating the formal sub-/metadiscipline in the 'modern' should also be found in them, but to a lesser extent than disciplinarity and disciplining. Uses of 'knowledge', 'discipline', 'represent' and 'modern' were counted. All appearances of each and its derivatives were included, even colloquial usages like 'to my knowledge', and regardless of the referents of 'discipline', 'represent', and 'modern'. The results are shown in Table 6.16.

Table 6.16: Mean standardised use of 'knowledge' (K), 'discipline' (Di), 'represent' (R), and 'modern' (M) over six decennia, with numbers of using presidents (n).

Period	K	n	Di	n	R	n	M	n
1935-1944	1.22	9	0.24	4	0.66	10	0.48	7
1945-1954	1.70	10	0.72	7	0.77	8	0.53	8
1955-1964	1.29	8	0.33	6	0.47	9	0.29	5
1965-1974	1.25	7	0.28	6	0.54	8	0.74	9
1975-1984	0.55	10	1.18	9	0.35	6	0.34	6
1985-1994	0.45	7	0.89	6	0.40	9	0.44	8
Means/totals	1.08	51	0.61	38	0.53	50	0.47	43

By both the extent of its usage and the number of using presidents, 'knowledge' (1.08, 51) is comparable with the two last terms in the vocabulary, 'values' (1.18, 57) and 'role' (1.18, 48), while 'discipline', 'represent', and 'modern' score well below that. So the prominence of disciplinarity and disciplining in the addresses is not matched by explicit attention to subdisciplinary orderings of them.

Content-Analysis: Reprise This content-analysis of the addresses was intended to show quantitatively that they can be treated as tacit sociologies of sociological knowledge, and to prepare the ideal factors in them for linkage with the real factors. In the first, the expected disciplinarity and disciplining have been found in the addresses. As seen in use of the 'vocabulary' and of the first person, they are both from sociology and a generically distinct and reflexive coverage of sociology; that genre was shown to entail elements of the epideictic, forensic, and deliberative genres of classical rhetoric. Finally, the antimetabole of the thesis was provisionally validated; the addresses are a sociology of knowledge, but tacitly so.

As for the second objective, the ordering of each president's address throughout this section allows the possibility of a linkage of its contents with his/her real factors. While the actual matching is deferred until the other set of ideal factors have been separated, both the likelihood of a fit and its potential jaggedness can be shown. A familiar pattern was found in the decennial mean totals in the content-analysis. This is shown in Table 6.17.

Table 6.17: Decennial mean totals (T) from content-analyses

Period	T
1935-1944	70.48
1945-1954	81.44
1955-1964	73.59
1965-1974	66.54
1975-1984	63.35
1985-1994	64.61
Overall mean	70.00

Recurrence of that pattern of rise and fall expected from accounts of American sociology, and found throughout the analyses of real factors, makes it likely that the two sets of factors can be matched. But separation of the presidents with the highest and lowest scores - overall and in the individual counts - shows that any matching will be messy. Highest and lowest overall scorers are shown in Table 6.18.

Table 6.18: Presidents with highest (CH, descending order) and lowest (CL, ascending order) scores from content-analysis

CH	CL
Gans {1988}	Sutherland {1939}
Znaniecki {1954}	Erikson {1985}
Blumer {1956}	Lipset {1993}
P. Rossi {1980}	Gamson {1994}
Sorokin {1965}	Hawley {1978}
D. Young {1955}	A. Rossi {1983}
MacIver {1940}	Merton {1957}
Kohn {1987}	Hughes {1963}
Parsons {1949}	Becker {1960}
Davis {1959}	Wilson {1990}

As suggested particularly by the appearance of Parsons and Merton in opposite columns, and more generally by both the comparable recognisability of the two sets of names and the spread of presidents within each, whatever matching is found between the real factors and the content-analysis will not be straightforward. That is even more evident when the highest and lowest scorers in each category of the content-analysis are separated. In the full display in Table B.7 in Appendix B (where low scorers were omitted when more than five presidents scored zero), 36 of the 60 presidents appeared in the fourteen categories of high scorers, and 30 in the ten categories of low scorers; only

10 presidents did not appear at all. Since presidents variously associated with Harvard/Columbia are concentrated in this last group - Stouffer {1953}, Homans {1964}, Moore {1966}, Goode {1972}, Komarovsky {1973}, Coser {1975}, and Coleman {1992} - it does suggest a disciplinary mainstream, or a zone of taken-for-grantedness. But that must be located within a swirl of more contradictory results.

So while the content-analysis does appear to have a certain validity despite its crudity/austerity, its civilisational limits are also apparent. A similar point is reached in the citation-analysis.

Citation-Analysis

Whereas content-analysis was developed beyond the subdiscipline, linked only through Merton and Lazarsfeld's (e.g. 1943) studies of propaganda, both the patterning of disciplinary texts through citation-analysis and the contesting of it have been at the core of Mertonian and post-Mertonian sociologies of knowledge. Since use of it is then in itself a form of subdisciplinary location, the method requires a fractal cutting down to size. That was resolved here along actor-network theoretical lines. The recognition of 'ideas' or 'influence' in Merton's model was bracketted, and references and citations were taken instead in the same sense as were the terms and repertoires in the content-analysis, as markers in networks under construction. They were treated too with the same crudity or austerity. No judgements were made as to whether references were substantive, affiliative or critical, and no distinctions were drawn between references and citations; thus a casual use of, say, 'Durkheimian' was treated as identical to a formal citation of one of Durkheim's texts. Even on such restrictive criteria, 'fairly coherent' patterns were found in the addresses

Three groups of writers were treated: the American and European 'classical' sociologists; the presidents themselves; and non-sociologists. A fourth category, of contemporary, non-presidential, sociologists was considered, but since the presidents referenced increasingly widely an unmanageable list of writers was generated, and the attempt was abandoned.⁷ The same procedure was followed in each analysis: counts were made of referencing presidents, and multiple references to a single author within any one address were ignored. Results from the three analyses are presented separately and then grouped for discussion.

⁷ That widespread referencing is consistent with Pomerantz's (1979) and Mulkay's (1984) findings on the 'reassignment' of praise.

The 'Classics' On the standard account, 'the discipline' was consolidated around a grafting onto a native tradition of Parsons' reading of European writers. To trace that process, references to the foundational American and European sociologists were counted, with attention restricted to the ten most mentioned in each category. The original selection of Americans was based on Faris' (1953: 103) listing of authors whom Parsons (1951) did not consider in *The Social System*; as a philosopher rather than a sociologist, Dewey was excluded, but Mead, also not formally a sociologist, was kept. After a first search, Giddings and Odum, cited in the presidential addresses but falling outside the top ten, were deleted from Faris' list, and Ross and Small added. Among the Europeans, those commonly included in histories of the discipline were counted, even if they did not identify themselves as sociologists. The two lists of 'classical' writers and the numbers of presidents referencing each are shown in Table 6.19.

Table 6.19: Numbers of presidents referencing the classical American (Am) and European (Eu) sociologists.

	Am		Eu
Park	16	Durkheim	22
Cooley	12	Marx	16
Ogburn	11	Weber	16
Thomas	11	Comte	9
Burgess	10	Simmel	7
Sumner	10	Pareto	6
Ross	7	Toennies	6
Mead	6	de Tocqueville	5
Small	6	Spencer	5
Ward	6	Tarde	4
Totals	95		96

The similarity in totals is striking. The difference in concentration of references to the two groups will be seen below to be matched by the expected difference across time. It should also follow from the standard account of American sociology that those most identified with the consolidated core would be most referenced by other presidents.

Presidents Referencing the Presidents Mentions of other presidents in each address were counted, on the same criteria as above but with self-citations and references to the presidents as editors rather than as writers excluded. The ten most referenced, and eleven unreferenced, are shown in Table 6.20.

Table 6.20: Most referenced (Re) and unreferenced (Un) presidents.

Re		Un
Parsons {1949}	14	Fairchild {1936}
Merton {1957}	13	E. Faris {1937}
Coleman {1992}	10	Hankins {1938}
Lazarsfeld {1962}	9	Sanderson {1942}
Homans {1964}	8	Vance {1944}
Blau {1974}	7	D. Young {1955}
Davis {1959}	6	R. Faris {1961}
Lipset {1993}	5	Hawley {1978}
Sorokin {1965}	5	Whyte {1981}
Turner {1969}	5	Liebertson {1991}
		Gamson {1994}

Given Parsons' and Merton's heading of the first column, the presence in it of many of their students and colleagues, and the general difference in recognisability of the names in the two columns, mutual referencing does indicate consolidation. It is striking that nine of the ten most referenced presidents either taught at Harvard or Columbia or graduated from them; conversely, only one graduate from either, Hankins {1938}, appears among the unreferenced, and his presidency preceded the golden age. It might also be expected that the more centripetal the discipline, the more recursive referencing would be. To test for that possibility, references to 'non-sociologists' were counted.

Non-Sociologists Identification of 'non-sociologists' was at first tentative. Since a daunting list of possibles was drawn from the presidents' texts and bibliographies, all those not mentioned twice or more were discarded. The affiliations of the remaining thirty could be located with more confidence, being associated with philosophy, 'science', or one of the other social sciences. Since it does not make sense to talk of academic 'disciplines' before the mid- to late-nineteenth century, all writers from before that time were sorted as 'classics'. Results are shown in Table 6.21.

Table 6.21: Non-sociologists mentioned twice or more.

<u>Classics</u>	<u>Anthropology</u>	<u>Philosophy</u>	<u>Psychology</u>
Newton (6)	Linton (6)	Dewey (4)	Freud (10)
Hobbes (5)	Kroeber (3)	Nietzsche (2)	Lewin (3)
Plato (4)	Malinowski (3)		Newcomb (3)
Aristotle (4)	Warner (3)	<u>Political Science</u>	Allport (2)
Adam Smith (4)	Boas (2)	Lasswell (3)	Fromm (2)
Galileo (3)	Murdock (2)	Almond (2)	
Bacon (3)	Radcliffe-Brown (2)	Dahl (2)	<u>Science</u>
Occam (2)			Darwin (8)
Machiavelli (2)	<u>History</u>		Einstein (5)
	Toynbee (3)		Gould (3)
			Bridgman (2)
			Lorenz (2)
			Mendel (2)

Given the permanent tension between sociology and economics, it is striking that no economists qualified for inclusion. But the extent of referencing across other disciplines - scores at the top of most categories bleed into those of the classics and of the presidents themselves - shows the contingency of disciplinary boundaries. Shifts of those boundaries across time are evident when this analysis is combined with the other two.

Citation-Analysis: Reprise To simplify comparison, the three analyses were recast by a separation of referencing presidents into decennial cohorts and then collected. Results are shown in Table 6.22.

Table 6.22: Numbers of presidents referencing American (Am) and European (Eu) classical sociologists, other presidents (Pr) and non-sociologists (Ns), across six decennia.

Period	Am	Eu	Pr	Ns
1935-44	7	5	4	7
1945-54	8	4	3	7
1955-64	7	8	9	9
1965-74	7	9	8	8
1975-84	4	7	10	8
1985-94	3	7	10	5
Totals	36	40	44	44

While the four totals are roughly similar, and while a rough trend is evident in each column, the four patterns suggest different effects. Peaking at about the times and in about the sequence expected on the orthodox account, references to the American and

European classical sociologists show 'the tradition' under the expected Parsonian-Mertonian reconstruction. But the general increase in mutual referencing, culminating in all presidents in the last two decennia mentioning at least one of their predecessors or successors, might indicate consolidation rather than the expected fragmentation of that synthesis. Conversely, the rise and fall of references to non-sociologists might suggest that sociology was most cross-disciplinary when it was supposedly most disciplinary.

That sets a limit to this citation-analysis. Since the meanings attributable to references are uncertain, and since use of the method entailed a bracketting of them, no more can be claimed than that 'fairly coherent' differences are present. Any suggested meaning could be countered. Civilisational attributions of 'ideas' or 'influence' cannot be separated from cultural invocations of disciplinary folk heroes. While mentions of the American and European classical writers, of the presidents themselves, and of non-sociologists do show that even crudely operationalised references can serve as markers of networks, the construction of those networks is another matter. So, having been undertaken within similar constraints, this citation-analysis has resulted in the same simultaneous suggestiveness and preclusion as found in the content-analysis.

Presidential Addresses - Ideal Factors: Reprise

The ideal factors, or features of the presidential addresses, were expected first, to validate treatment of the addresses as sociologies of sociological knowledge, and then to be patternable in such a way that a relation with the presidents' real factors could be established. As seen in the content-analysis, the presidents' language is consistent with both the overall model of disciplinarity-disciplining-discipline and the detailed processes within it. As seen in the citation-analysis, the presidents' references are consistent with a sociological tradition always under reconstruction. On either basis, the addresses are effectively sociologies of sociological knowledge. Or, they can be plausibly represented as such even if the limits of each method preclude more definite claims; the bracketted meanings of words in the content-analyses and of references in the citation-analyses cannot be restored in their results.

But since the 'fairly coherent' patterns found in the ideal factors are also as raggedly consistent with the orthodox account of American sociology as were those in the real factors, it is plausible to follow the defining imaginary of the sociologies of knowledge and attempt to establish relationships between them. That is undertaken

through correspondence-analysis.

REAL AND IDEAL FACTORS - CORRESPONDENCE-ANALYSIS

The likelihood that the presidents' organisational characteristics could be matched with the characteristics of their addresses was evident throughout the previous section, since the patternings of the ideal factors resembled those of the real, and since both were raggedly consistent with standard accounts of American sociology. At the same time, the methods used are interpretively and operationalisationally limited. Given the uncertainties of the context and genre from which the real and ideal factors were drawn, and given that they were separable only on the condition that meanings were bracketted, any attempt to treat them as well-defined independent and dependent variables would entail Coser's (1975) 'fallacy of misplaced precision'. But the impossibility of finding statistical correlations between real and ideal factors does not preclude an inductive ordering of them, as a qualitative test and as a preliminary to more detailed study. Correspondence-analysis is one means by which the two sets of factors can be reconciled in those terms.

Correspondence-analysis is a method for inductively locating latent patterns in large multiway tables, and since it can resolve in a single analysis the heterogeneous results of coding, raw scores, and frequency counts, it is well-suited to the findings here. A correspondence-analysis of a matrix of data is a weighted principal components analysis, in which correlations between and within rows and columns are maximised and expressed as 'axes of inertia' or 'dimensions'. These might be envisaged as planes of best fit intersecting in n-dimensional space, with the 'positive' and 'negative' variables in each plane having the sense of Cartesian coordinates. Or, correspondence-analysis is a form of n-dimensional Chi-square testing, and while inferential results are invalid when data are not frequencies, it still allows a descriptive account of any underlying structure in them (Weller and Romney 1990; Greenacre 1993; Patman 1993; Phillips 1996). That is ideal for the limited claims possible for the two sets of factors here. It is expected that their combination will yield 'dimensions' which are effects, jointly and raggedly, of the model of the disciplinary and the reading of 'the discipline'.

The real and ideal factors were then collated in a multiway table. Given the sources of the data, this table is a 'disciplinary matrix', both literally and in that sense

taken from Kuhn which was one source of fractal ordering. Thirty-one dimensions were found in correspondence-analysis of it. The percentages 'explained' by the leading five are shown in Table 6.23.

Table 6.23: Percentage of total variability 'explained' by five leading dimensions (X, percentage; Y cumulative percentage).

Dimension	X	Y
1	16.5	16.5
2	12.8	29.3
3	11.1	40.4
4	7.4	47.7
5	6.3	54.0

Now beyond whatever latent patterns may be present, the number of dimensions and the percentage of variability 'explained' by each in a correspondence-analysis are also effects of the size of the matrix. Criteria of 'success' are then not clear when, as here, the use of Chi-square tables is precluded. In her analysis of a 4-by-4 table, for example, Phillips (1996) found a first dimension accounting for 93% of variability, and could unproblematically assume its salience. But claims vary when matrices are much larger. In one of the best known correspondence-analyses, Bourdieu (1979: 263) drew conclusions from three leading factors which represented only 5.8%, 3.6% and 3.2% of variability, while in his actor-network theoretical study of 'co-words' Courtial (1986: 191) found correspondence-analysis unsuitable for his purposes on the grounds, *inter alia*, that seventeen dimensions were required to account for 50% of total variability; his three leading factors had scores of 6.3%, 4.9% and 4.1% respectively.

Results here are more encouraging, since the three leading dimensions account for over 40% of variability. Especially in view of the crudity of the data, it is striking too that they yield a 'fairly coherent' matching with triadic fractal ordering. Each of them is examined briefly before the correspondence-analysis overall is summarised. In each of these sketches the top and bottom 10% of variables and presidents are listed. The scores are Cartesian coordinates; positive and negative contributions to each dimension are given in descending and ascending order, respectively.

Dimension 1: Discipline and collectivity

The variables and presidents contributing most to this dimension, which 'explains'

16.5% of total variability, are shown in Table 6.24.

Table 6.24: Variables and presidents contributing to Dimension 1.

Variables		Presidents	
+ve	-ve	+ve	-ve
First pers. pl.	Columbia	MacIver {1940}	Blau {1974}
Discipline	1965-1974	E. Faris {1937}	Lipset {1993}
1945-1954	Harvard	Blalock {1979}	Vance {1944}
Chicago	Eu. Classics	Gans {1988}	Yinger {1977}
		Queen {1941}	Hawley {1978}
		Liebertson {1991}	Coleman {1992}

Three points are notable here. First, it is reassuring in a study of the disciplinary that 'discipline' emerges as a leading variable, and that it is positively associated as well with the 'collectivity' suggested by use of the first person plural. Secondly, the contrast over time between 'Chicago' and 'Harvard/Columbia' is clear. The advantage of correspondence-analysis, thirdly, that it can result in non-intuitive links, is apparent in that only one of the twelve presidents in Table 6.24 held office during the two decennia found significant. Since results are overall effects rather than specific associations, this accommodates the melange of presidents found in, say, Table 6.2 in the cohort-analysis or Table 6.18 in the content-analysis.

Dimension 2: Science and Knowledge

This dimension accounts for 12.8% of total variability. The leading variables and presidents contributing to it are shown in Table 6.25.

Table 6.25: Variables and presidents contributing to Dimension 2.

Variables		Presidents	
+ve	-ve	+ve	-ve
1935-1944	Gender F	Merton {1957}	Blumer {1956}
Science	Empiricist	E. Faris {1937}	Thomas {1952}
Knowledge	1985-1994	MacIver {1940}	Kohn {1987}
Harvard	First pers. sing.	Wirth {1947}	Wilson {1990}
		Hankins {1938}	Lazarsfeld {1962}
		Lundberg {1943}	P. Rossi {1980}

While it is again reassuring that key terms from formal study emerge as salient, it is clear that 'science' and 'knowledge' - 'discipline' fell just outside the leading *negative*

variables - are polyvalent. The contrasts between the first and last decennia and 'science' and the 'empiricist' repertoire suggest a difference between invocation and use of the 'scientific'. That is shown further by the presidents, for while only perhaps Lundberg among those in the positive column was known for the 'scientificity' of his work (Merton appears here through his treatment of science as topic), the negative column contains several who were. The agonistic character of the repertoires must also be stressed; Blumer's ranking, for example, is due to his critique of 'variable analysis' rather than to endorsement of it. Finally, the link between female presidents and the empiricist repertoire suggests that here at least a specifically feminist methodology was not an issue.

Dimension 3: Discipline and the Golden Age

This third dimension 'explains' 11.1% of total variability. Its constitution is shown in Table 6.26.

Table 6.26: Variables and presidents contributing to Dimension 3.

Variables		Presidents	
+ve	-ve	+ve	-ve
Science	1985-1994	Merton {1957}	Goffman {1982}
1955-1964	First pers. pl.	D. Young {1955}	MacIver {1940}
Discipline	1945-1954	Lee {1976}	Erikson {1985}
Knowledge	1935-1944	Bendix {1970}	K. Young {1945}
		Taylor {1946}	Wirth {1947}
		Short {1984}	Angell {1951}

The polyvalence of 'science' is even more evident here. Associated now not only with 'knowledge' but with 'discipline' as well, and with the latter opposed to rather than linked with use of the first person plural, it entails a different temporal structure and a different alignment of presidents. The contrast between the third decennium, on the one hand, and the first, second and sixth decennia, on the other, suggests that reaching of scientific maturity in sociology claimed for the golden age and then that fragmentation said to have followed it. As confirmed by the move of MacIver and Wirth from positive association with Merton in the second dimension to a negative linkage here, a quite different sense of 'science' is being measured. The association of scientific knowledge with the golden age shows too a different aspect of 'discipline'.

So the patterns within the three dimensions allow 'fairly coherent' links to be

drawn with both the formal sociologies of knowledge and accounts of 'the discipline'. Despite all the limits on their operationalisation, real and ideal factors do interact in a more or less 'orderly sequence'.

Real and Ideal Factors - Correspondence-Analysis: Reprise

The real factors of the presidents' organisational characteristics and the ideal factors of their addresses are interpenetrative. Consisting of balanced polarities, the dimensions in the correspondence-analysis are given interpretively as zones of contestability rather than as fixities, and it is then not surprising that those here have elements in common. But it is striking that not only are there three leading dimensions, but that they are multifaceted representations of both 'discipline' and 'the discipline'.

The dimensions are broadly consistent with accounts of American sociology. Among the real factors, 'school', 'gender' and 'cohort' were particularly salient. The contrast between 'Chicago' and 'Harvard/Columbia' confirmed here is a disciplinary cliché. The association between female presidents and the 'empiricist repertoire' is consistent with claims of a 'missing feminist revolution' in sociology and thus with the maintenance of a disciplinary core despite a significant change. But as the recurrence and shifts of the decennial cohorts suggest, that core is itself unstable: the timing of the contrast between 'Chicago' and 'Harvard/Columbia' in the first dimension gives one version; the opposition of the first and sixth decennia in the second dimension suggests a linear development; but identification of the golden age in the third dimension allows an alternative reading.

The findings in the first and third dimensions are supported, furthermore, by the absence throughout of the fifth decennium, 1975-1984, a time when disciplinary fragmentation was widely diagnosed. That instability is confirmed by the presidents contributing most to each dimension, and by the reappearances among them. The spread of times and backgrounds among the thirty presidents - half the sample - shown in the three tables does not allow any simple matching with any periodisation of 'the discipline'. Similarly, of those who appear more than once, Merton {1957} and perhaps Wirth {1947} would be accepted as major disciplinary figures, but the claim would be made with less confidence for Ellsworth Faris {1937} or MacIver {1940}, the most disciplinary president here. Overall, then, the three dimensions can be matched with 'the discipline', on the proviso that any number of problems have been glossed. But the

fractal ordering of the disciplinary does offer some purchase on these.

For the linking of real and ideal factors in this chapter, it was hypothesised that presidential addresses were generically distinct effects of the disciplinarity, disciplining and discipline of sociology, and that they amounted to a folk sociology of sociological knowledge. That is supported in the correspondence-analysis. Despite its relatively scant use throughout the addresses, 'discipline' was a key term in the first and third dimensions, and all but the same in the second, and in ways roughly consistent with fractal ordering. The collective moment of disciplinarity, and with it the generic distinctiveness of presidential addresses, was given in uses of the first person plural and of 'discipline' being the leading positive contributors to the leading dimension. Other perspectives on the scientific moment appeared in the other two dimensions, and there too the concern with sociological knowledge was evident. All that validates the fractal ordering of the addresses as effects of 'discipline'. But elements of the model are missing. The useful moment of disciplinarity did not emerge, while the disciplining indexed by the 'normative repertoire' appeared to be as non-significant as, say, the presidents' disciplinary ages or 'embeddedness'. Now, while the absence of disciplining fits the proposal that the presidents are situationally debarred from what what is situationally required of them, it might just as easily be an effect of an inadequate index. Similarly, usefulness might either have been badly measured or be so taken for granted that it does not emerge as contestable. Once such alternatives are equally possible, the limits of this correspondence-analysis have been reached.

While the three leading dimensions drawn from it are 'fairly coherent' under expectations of both 'the discipline' and 'discipline', only modest claims can be made for them. They accounted, for one thing, for only 40% of the variability in the 'disciplinary matrix'. Then, the crudity of the data might make such ordering as has been achieved all the more striking, but it also restricts interpretation. The statistical testing of inductively found patterns, a common next move from correspondence-analysis, is precluded, and it cannot be decided whether or not absences from the findings are significant. The most that can be claimed is that there are non-random associations between real and ideal factors, and that these are roughly consistent with fractal ordering. More detailed examination requires non-quantitative methods.

THE PRESIDENTS AND PRESIDENTIAL ADDRESSES - REAL AND IDEAL FACTORS : REPRISE

Classical sociology of knowledge entailed identification of what Scheler (1925: 36) called "the orderly sequence of the *reciprocal effects* of ideal and real factors", and throughout the history of the field quantitative methods have either been urged or employed toward that end. On a shift from 'orderly sequence' to 'fractal ordering', the imaginary of scientificity given in them was followed in this chapter as well. It was intended, first, to show that 'fairly coherent' patterns could be found when Scheler's civilisational/cultural distinction of real from ideal was lifted, but that these were possible only if interpretive closure was foregone. Then, those patterns were to be used as a test of the reading of American sociology in the previous chapter while at the same time the reading was a test of the fractal ordering from which it was derived. Those objectives were met in the separate analyses of metaphorically understood real and ideal factors and in their attempted reconciliation in correspondence-analysis.

Recurrent patterns emerged when the presidents' real factors were ordered by decennial cohorts. Regularities in the presidents' disciplinary and/or actual ages, in the schools from which they graduated, and in their organisational embeddedness were raggedly consistent with that centripetal phase in American sociology, the Parsonian-Mertonian golden age, and with the fragmentation which followed it. But the fit was only ever partial, and as seen in the ASS/A's bifurcated honorific structure and in the increasing prominence of women, the centripetal was open to centrifugal re-definition. The formal disciplinary dynamics beneath the patterning required supplementing with a reading of 'the discipline' as a moral community. As suggested in 'disciplinary justice' or in the uncertain relation between organisational 'success' and disciplinary 'merit', epistemological judgements were always already infused with the political, the civilisational with the cultural.

Similar results were found in the ideal factors in the presidential addresses. Definite findings were achieved: in the content-analysis, for example, use of the first person showed the addresses as generically distinct, use of the 'vocabulary of sociology' showed them as disciplinary, and use of 'sociology' showed their reflexivity, all steps towards treatment of them as sociologies of knowledge. The pattern of rise and fall in both the content- and citation-analyses was again that expected from accounts of the golden age won and lost. But formal dynamics were again incomplete. In the content-

analyses, for example, uses of particular terms or repertoires could not easily be linked with formally linked presidents. Referencing patterns might suggest lines of 'influence' in the discipline, but they might also show invocations of disciplinary folk heroes. Here again is that mix of the formally suggestive and the interpretively messy.

That, finally, was made explicit in the correspondence-analytic linking of real and ideal factors. The presence of non-random associations does demonstrate existential determination, and in ways which circularly validate the reading of 'the discipline' and the fractal ordering brought to its study. But less than half of the variability within the disciplinary matrix of factors was 'explained', and that explanation was limited as much by the crudity of the data which made it possible as by the range and mix of presidents linked with the associations. Overall, then, quantitative analyses end in both moderate support for fractal ordering and a demonstration of the necessity for the other methods included in it.

'THE DISCIPLINE' : REPRISE

This second part of the thesis has been intended to meet two layered aims. Within the structure and argument of the thesis, that sociologies of knowledge are representations of discipline and that representatives of 'the discipline' are sociologists of knowledge, it is, first, a demonstration that as disciplinary artefacts presidential addresses comprise a strategic research site, and then a setting of the context for their reading. At the same time, that demonstration and that setting are themselves both an enactment and a test of the fractal ordering under development. To meet the second pair of aims, the two chapters of Part II were structured by the methodological moments set in Part I. The reading of American sociology in Chapter 5 followed from the assumption that disciplinary networks are 'narrated like discourse', the quantitative analysis in this chapter from their being 'real like nature', and both entailed the reflexive and opportunistic enrolments given by the extension to textual ethnography of the networks' character as 'collective like society'.

More specifically, narration of 'the discipline' of American sociology was seen in Chapter 5 to be consistent with the modelling of 'discipline' as congenitally failing, the presidency of the ASS/A to be documentary of it, and presidential addresses in turn to be documentary documents. The generic distinctiveness of the addresses was seen to be

an effect of disciplinary exemplification, disciplinary policing, and disciplinary honour, and to entail, too, a situated debarment of a situationally required reflexivity which amounts to a lived version of 'Mannheim's paradox'. Those fractal matchings allow the addresses to be read as a folk sociology of knowledge. The quantitative analysis in Chapter 6 was undertaken as a test of that reading of 'the discipline' and the place in it of presidential addresses, and, circularly, as a further test of the extension from binary to trinary categories in the modelling. Again, 'fairly coherent' results were achieved; both the reading and fractal ordering were supported, but with a raggedness confirming the limits of quantitative analysis and suggesting that it be supplemented with rhetorical study. After the argument thus far in the thesis is summarised in a case-study of Merton {1957}, as a central figure in the modelling of 'discipline' in Part I and in the reading of 'the discipline' in Part II, a rhetorical examination of the addresses is developed in Part III.

CASE 6 : MERTON'S ANTIMETABOLE

Merton {1957} has been critical to both phases of the argument so far, as a leading sociologist of knowledge, and as a leading American sociologist. His own work and developments in the two fields were interwoven. The year in which he completed what was to become the noted subdisciplinary text of his thesis was also the year of disciplinary consolidation marked by the *ASR*'s foundation, and he was president of the *ASS* at the apogee of the golden age. That co-occurrence can be extended. Since his address on 'priority of discovery' is also cited as a founding subdisciplinary text, it is a limit-case in the fractal ordering of the thesis: as a sociologist of knowledge, Merton shaped a formal representation of discipline; as a representative of 'the discipline', he acted as a folk sociologist of knowledge. If presidential addresses are 'strategic research materials', in his sense (e.g. Merton 1987), then his simultaneous enactment of those two moments makes his address uniquely strategic within them.

Its doubly exemplary character will be treated in three stages. As formal sociology of knowledge, the address will be shown to entail Merton's familiar binary, an opening of it in 'ambivalence', and its match with 'the discipline'. Then, as address, it will be seen to fit the modelling of the disciplinary, and so to be a folk sociology of knowledge. The recurrence in it, finally, of the blunted reflexivity characteristic of presidential addresses will be used to reconcile the formal and folk sociologies.

The reading of sociologies of knowledge as representations of discipline was derived from the separation in them of the epistemological and political senses of representation, and since his account of science hinged on the distinction of social from epistemologically warranted cognitive norms, Merton was read as epitomising the process. He extended it in the 'chapter in the sociology of science' of his address, where he took disputes over priority as effects of the 'norms of science', but with the addition of an interactive third term between his civilisational and cultural polarities.

Under the norm of 'communism', Merton (e.g. 1942) had held that acknowledgement of priority in discovery was both scientists' only 'property-right' and a source of their motivation. Scientists, he said, were continually reminded that their role was to advance knowledge, and that 'in the institution of science, originality is at a premium'. This was evident in the hierarchy of rewards open to them; at the peak

were epochal terms like 'Newtonian', followed by naming as the 'father' of a discipline or speciality, then by recognition through eponymous laws, techniques, and so on, and finally by Nobel Prizes, other medals and awards, membership of prestigious societies, and the like. As a history of 'sordid' disputes showed, false or misplaced claims to priority, and thus to the rewards following from it, generated moral indignation, not only in the scientist so wronged but in others not personally engaged as well. These 'feel strongly about the single property-norm, and the expression of their hostility serves the latent function of reaffirming the moral validity of this norm'.

However, the norm of 'humility' which Merton now added to his list of 'the norms' conflicted with the quest for recognition of originality. Humility might take the form of an acknowledgement of indebtedness to predecessors, or of an insistence on either personal limitations or the limits of scientific knowledge generally. Merton held that the coexistence of the two norms led to 'ambivalence'. Since it was not humble to insist on priority, and since to take priority as unimportant was not to affirm originality, 'scientists come to despise themselves for wanting that which the institutional values have led them to want'. While not all resolved the tension within normative bounds, a 'culturally induced tendency toward deviation' was moderated by 'an emphasis on the value of truth by whomsoever it is found, and a commitment to the disinterested pursuit of truth'. Here is that separation of the epistemologically pure from the politically messy which marks the sociologies of knowledge as representations of discipline, but with a structurally induced 'ambivalence' now filling the gap between Merton's early polarities of civilisation and culture.

This account of a puzzling recurrence in science was both a subdisciplinary milestone and exemplary of 'the discipline'. For years it was the most cited paper by the most cited author in the field (Cole and Zuckerman 1975: 156). In it Merton "turned the sociology of science in a more empirical direction" (Collins and Restivo 1983: 188). It ranked with Kuhn's *Structure* in the shaping of the subdiscipline (Zuckerman 1988: 512). In terms of 'the discipline', Merton had used Parsonian categories in his original paper on 'the' norms of science, and the extension of them here through his own modelling of deviance made his address exemplary as well of the golden age; this was a time when Merton "situated himself at the crossroads of a discipline of almost mutually oblivious approaches, and thereby managed to come as near as anyone to

directing traffic" (Collins 1977: 150).¹ As an *imago* of the discipline as practised, the discipline of Merton's representation entailed a return of the politically repressed into the epistemologically warranted. Merton's meritocratic account - since rewards accrued to the most original, science enabled 'those happy circumstances in which self-interest and moral obligation coincide and fuse' - is paradigmatic of the modern.

Exemplary in both subdisciplinary and disciplinary terms, the paper then illustrates the argument that the sociologies of knowledge are representations of discipline, and that 'discipline' is paradigmatic of sociology's location in what is studied within it. It exemplifies, too, its 'congenitally failing' character. An oddity in the content-analysis, that despite his centrality to American sociology Merton was among the lowest users of the 'vocabulary' drawn from it, is echoed in the way that this exemplary paper did not meet the rigour of Merton's imagined science. He did not define, for example, the crucial 'discovery'; his evidence was illustrative, selective, and anecdotal, with none of the systematic sampling which might have been expected; and he extended his list of 'the' norms without either acknowledging that he was doing so or suggesting a typological exhaustiveness.² Since Mertonian theory did not account for Mertonian practice, the inclusion of 'ambivalence' in the former epitomises 'failure'. Whether or not what he was to say of Le Bon could be applied to him - "Like most men struggling with ambivalence, [he] evolved a reconciliatory doctrine" (Merton 1960: xxviii) - his proposal of an interaction between the norms of 'humility' and 'originality' was an opening of the dualistic preemptions of his early work. But with 'the disinterested pursuit of truth' still quarantined, his foreclosures remained. Recalling the 'early days of modern science' which sociology was laggardly retracing, 'sociological ambivalence' entailed the either/or of a quasi-Newtonian "dynamic alternation of norms and counter-norms" (Merton and Barber 1963: 104), rather than accommodating the both/and of the politically epistemological.

Now to use 'sociological ambivalence' here is to risk the psychological attributions which occasionally mark Merton's own usage. But as both a recognition and repetition of aporia the term does connote the failure of the discipline defended through it, and so sums up the exemplary standing of Merton's paper on 'priority'. The

¹ Turner's (1978: 83) description of the model of anomie and deviance as "[p]robably the most widely read and influential theory developed by Merton" or Cole's (1975: 175) claim that "[i]n undergraduate sociology courses it is still used as one of the finest examples of a sociological theory", could be matched from any number of other sources.

² Brannigan (1981) elaborated a constructivist reading of the difficulties in Merton's argument.

paper was also an exemplary presidential address.

A presidential address is generically a response to personal honour, exemplary sociology, and a policing of both the discipline's internal state and its relevance to the world at large; those interpenetrative effects are fractals of the modelling of the disciplinary into moments of disciplinarity, disciplining and discipline. That Merton's address can be read in those terms is evident in his introduction:

We can only guess what historians of the future will say about the condition of present-day sociology. But it seems safe to anticipate one of their observations. When the Trevelyans of 2050 come to write that history - as they well might, for this clan of historians promises to go on forever - they will doubtless find it strange that so few sociologists (and historians) of the twentieth century could bring themselves, in their work, to treat science as one of the great social institutions of the time. They will observe that long after the sociology of science became an identifiable field of inquiry, it remained little cultivated in a world where science loomed large enough to present mankind with the choice of destruction or survival. They may even suggest that somewhere in the process by which social scientists take note of the world as it is and as it once was, a sense of values appears to have become badly scrambled.

This spacious area of neglect may therefore have room for a paper which tries to examine science as a social institution ...

This is rhetorically presidential. Since the sociology of science was an 'identifiable field of inquiry' on the basis of Merton's own work, his claim is an epideictic recognition of the recognition of his achievement. As a 'chapter in the sociology of science', the address entailed a deliberative schema for continuation of that work. Given Merton's linking of the already morally weighted charges of 'neglect' and 'scrambled values' with the sense of crisis of the atomic age, it was forensically normative. That generic recognisability can be recast in terms of the modelling of the disciplinary.

The disciplinarity of sociology was defined as a performative imaginary of the application to problems in social order of collectively sustained and scientifically warranted knowledge. Merton's opening 'We' reflects the collective occasioning of the address, an effect the more striking for the finding in the content-analysis that he was among the lowest users of the first-person plural. As also shown there, Merton was the president most associated with 'science', and if that followed from his treatment of science as topic rather than as resource there is no doubt that he imagined sociology as scientific. The pattern of disputes and rewards he found in the physical and natural sciences 'is true also of all the other scientific disciplines, not excluding the social and psychological sciences': Comte, for example, had disputed priority with Saint-Simon, and was numbered ('of course') in the list of 'Fathers' of disciplines; or, the 'Lazarsfeld

latent-structure analysis' exemplified eponymy. Merton emphasised the usefulness of sociological knowledge less than its scientificity, but it is evident in his assumption that sociology had to be relevant to the major issues of the day. The address is then an expression of disciplinarity, which in turn was the basis for Merton's disciplining.

This moment of the disciplinary was explicit in Merton's charge of scrambled values, and implicit in his application to science (and thus sociology) of his account of deviance. Further, and as suggested in the subdisciplinary centrality of the address, this was an exercise in disciplining with tangible results. One strategic advantage of Merton's paper is that where the disciplining strain in presidential addresses is read throughout the thesis as a matter of performativity, there is evidence of its actual performance. Storer (1973: 282), for example, who was to edit Merton's papers in the sociology of science, remembered:

the waves of response that greeted the recital of paternity of the sciences, the various echelons of eponymies, and the varieties of misbehavior of scientists. (Given the direct bearing of the subject on many of the listeners' deep personal concern with their roles in science, the laughter may also have provided a vent for the anxieties that were building up as Merton explored some rules of the game and some of their consequences).

But there's the rub. In his recollection of the address, Tiryakian (1991: 509) held that it had "enthralled [its] audience as much for Merton's delivery as for [its] substance", and in that sense, the more successful Merton's disciplining was, the more it also suggests that permanently failing process definitive of 'discipline'.

Merton's stylish dismissal of stylish sociological writing was read in Chapter 2 as symptomatic of the difficulties in his distinction between culture and civilisation, and that pattern is repeated in the address. If it was a rhetorical achievement, then the more evocative the rhetoric, the more Merton undermined the 'science' of his disciplinarity. Since rhetorical effectiveness is by definition incompatible with 'the disinterested pursuit of truth', the address entailed a collapse of the separation of the civilisational from the cultural, or of the epistemological from the political.³ So the same point has been reached in a reading of the address, *qua* address, as in treatment of it as formal sociology of knowledge. It is now shown that the same mechanism is operative in the two genres.

That mechanism involves the blunted reflexivity characteristic of both the

³ This effect is even stronger in Merton's (1965/1985) 'Shandean postscript' to the address. Suggesting the 'failure' here, Simon and Gagnon (1967: 425) read this as symptomatic of Merton's "frustration with the sociology he helped fashion".

sociologies of knowledge and presidential addresses. Deriving from similar sources, it leads to similar preemptions. One recurrent move found in the sociologies of knowledge was the assumption of epistemology as foundational, with the corollary quarantining of it from the political, and with the effect of foreclosure on the sociologising of sociology. Merton's work epitomises that process. Through all the variations on his distinction of civilisation from culture which recur from his earliest to his latest papers, this leading sociologist of knowledge and this leading American sociologist preempted, precisely, his sociology. This was not a personal pathology. On a rhetorical reading of both 'discipline' and 'the discipline', Merton was a leading figure because of his persuasive articulation of common sense. As seen in any number of writers' mockery of Mannheim, for example, it was risible to treat epistemology as anything but foundational. The interrupted reflexivity of the subdiscipline is a feature of the discipline at large.

It is a feature of presidential addresses as well, as an effect of their generic specificity. They are responses to personal honour, but the presidents must moderate their situationally required agreement that they are worthy. They are policings of the discipline, but the presidents are constrained by their audiences' expectations. They are exemplary sociology, but the presidents are faced always with alternative and perhaps equally convincing practices. All that amounts to an occasioned debarral of what is occasioned as required. The presidential address, too, entails the blunted reflexivity which follows from appeals to disciplinary common sense. It is in that sense that Merton's address exemplifies both formal and folk sociologies of disciplinary knowledge. As in every other address, his foreclosure took an idiosyncratic form. The 'neglect' of the study of science with which Merton charged his listeners, for example, was his own; the sociology of science may have been 'an identifiable field of study', but beyond another paper on 'neglect' (Merton 1952) he had done virtually no work in it himself since his thesis and his papers on the normative order. But if the expression was particular, the effect was generic. It is because of that recurrence that if sociologies of knowledge are representations of discipline then representatives of the discipline act as sociologists of knowledge.

The interruption of reflexivity meant that Merton's theory did not account for Merton's practice. But once his foundational claims for epistemology are foregone, his practice is a model. As a master of disciplinary rhetoric, he exemplifies approaches to

difficulties he also exemplifies. In this thesis, for example, presidential addresses are being taken as Mertonian 'strategic research materials', the presidency as part of the Mertonian 'reward system' of the discipline, and sociological knowledge as being as normatively patrolled as in the Mertonian model. The "difficult trick in the art and craft of science", Merton (1985b: xi) said, "is to exercise discipline while still obeying one's daimon", and so long as his own shift from daimon to purification is avoided, that remains a model.

PART III:

REPRESENTATIVES OF 'THE DISCIPLINE' AS SOCIOLOGISTS OF KNOWLEDGE

To complete the antimetabolic study of disciplinary rhetorics, the presidents of the ASS/A - formal representatives of 'the discipline' - will be shown in this third part of the thesis to act as folk sociologists of knowledge. The amenability of their addresses to fractal ordering suggests a resolution of the peculiarities of sociology's location in the processes studied within it.

Sociologies of knowledge were shown in Part I to be representations of the disciplinary. 'Fractal ordering' was introduced to describe the recurrently advanced trinary alternative to the just as recurrent binary distinction of the epistemological from the political senses of representation. The disciplinary was modelled as a fractal effect of disciplinarity, disciplining and discipline, where disciplinarity was defined as itself a fractal *topos*, comprising optimistic imaginaries of collectivity, science, and usefulness, disciplining as the purifying work of their invocation, and discipline as the congenitally failing effect of their interaction.

Part II was both a test of that model and a contextual setting for comparison of formal and folk accounts. It was shown that 'the discipline' was consistent with the modelling of 'discipline', that presidential addresses to the ASS/A were documentary of it, and that these were presented through the repertoires of disciplinarity, disciplining and discipline expected from the formal sociologies of knowledge. That showed a match between fractal ordering and the genre of presidential address. It was suggested further that the addresses were a lived version of 'Mannheim's paradox', since the presidents were situationally debarred from a situationally required reflexivity. But since appeals to disciplinary common sense are a rhetorical resource adequate for all practical purposes, the presidents, formal representatives of 'the discipline', can be seen to act as folk sociologists of sociological knowledge.

That will be demonstrated in the three chapters of Part III, through the use in

the addresses of the disciplinary rhetorics modelled in Part I: Chapter 7 is devoted to the folk disciplinarity of 'the discipline', Chapter 8 to its folk disciplining, and Chapter 9 to its folk discipline.

Since the presidents at least touched on all the major substantive concerns of American sociologists, their topics, in a conventional sense, might be used to index shifts in disciplinary focus under shifting conditions. But since that sociology of knowledge is precluded here, by the difficulty of matching 'real' and 'ideal' factors, 'the topics' are taken in Chapter 7 in their rhetorical sense. As *topoi* of disciplinarity, the imaginaries of collectivity, science and usefulness are shown, first, to recur in the addresses, then to be jaggedly conflictual, and so to require implicit resolution.

The presidents' tacit adaptation to occasioned constraints will be treated in Chapter 8, on folk disciplining. Presidential addresses are generically normative, but to a generically muted degree. They entail appeals to 'the topics' of disciplinarity, but these are limited by the conflictual taken-for-grantedness in each of the collective, scientific and useful imaginaries. So it is suggested that much of the presidents' disciplining is enacted through metaphorical and intertextual links with other forms and sources of social order. That effect will be detailed through their use of one image, that of 'task'.

Since use of 'task' will be seen as fractally blurring the disciplinary bounds of the disciplinary knowledge under defence, the basis for a reading of the 'failure' expected of 'folk discipline' will have been set. That will be elaborated in Chapter 9, where the presidents' explicit attention to failings in the documents, devices and drilled bodies of the discipline and their implicit accommodations of them are read against their use of 'discipline', 'modern', and 'knowledge'; when they depict sociology as a 'young science' which might be susceptible to 'paradox' and which is a matter of 'representation', the presidents show the essential tension in separation of the epistemological from the political.

The fit between their addresses and the modelling of the disciplinary then shows the presidents, formal representatives of 'the discipline', as folk sociologists of disciplinary knowledge.

CHAPTER 7: FOLK DISCIPLINARITY

A reading of presidents of the ASS/A as folk sociologists of knowledge requires first that their addresses be shown to entail a folk 'disciplinarity'. This fractal *topos* of a relatively natural disciplinary worldview epitomises the location of sociology in the processes studied within it, since the technical rationality, the constructive visions of the social, and the affective bonds definitive of it are not limited to the 'scientific community' of sociology. Its salience in 'the discipline' and in presidential addresses having been shown in Part II, its functioning as a disciplinary commonplace will be illustrated in this chapter through representative passages taken from the addresses.

After a note on the topics of the addresses, in the conventional sense, each of the *topoi* of disciplinarity will be treated separately. First, there is no doubt that the addresses are effects of the 'collectivity given in their occasioning: the presidents' use of the first person plural was shown to be generically distinct, and, associated with 'discipline', that usage appeared as the leading term in the leading dimension of the correspondence-analysis. To stress the addresses' suffusion with 'collectivity', it will be treated twice, in an introductory note and in a more detailed reprise of the other two topics. The next of these, 'science', is just as clearly a disciplinary imaginary; the presidents use the physical and biological sciences for performative metaphors and for models of the methods they use to claim that sociological knowledge is lawful. On 'usefulness', thirdly, the presidents appear to assume at least the contestability of sociology's standing as a policy science. Once 'science' and 'usefulness' have been shown, attention will return to 'collectivity'. Consistently with the sense of *topos* as a source rather than resolution of arguments, the presidents represent the fusion of the other two topics as a disciplinary tradition, a disciplinary danger, or a disciplinary opportunity; in each case, the collectivity they imagine belies their presumptive separation of epistemological and political representations. The appearance in the addresses of the trinitarian topics of disciplinarity is jaggedly consistent with the formal sociologies of knowledge. As in those sociologies, furthermore, but more explicitly so, Bacon appears as a focal point.

But it must be stressed that the three moments are as amenable to disparate and even contradictory use as are all rhetorical topics. Just as Aristotle held that either side of any case could be argued validly from the commonplaces, so the presidents turn

'collectivity', 'science', and 'usefulness' to very different ends. This characteristic will be used to account for the peculiarity noted in Part II and in the various case-studies, that while many presidents had worked in the sociologies of knowledge, few made explicit use of them. The topical structure of the addresses is an effect of the situated debarral of the situated requirement for disciplinary reflexivity, or of their occasioned enactment of 'Mannheim's paradox'. The disciplinarity evident in the addresses is then a first step towards representation of them as folk sociologies of knowledge.

TOPICS AND THE TOPICS

Since most of the substantive concerns and theoretical orientations of 'the discipline' can be found in presidential addresses, their topics, in a conventional sense, could be treated through a sociology of knowledge in which empirical interests were linked to historical conditions. The presidents' work has been used to chart long-term patterns in both the discipline itself and its relation to the world at large: Peter Rossi (1980), for example, treated shifts in the distinction of pure from applied sociology through his predecessors' work. But such a sociology of knowledge is precluded here. Since historical accounts emerge from agonistic networks closely related to those of sociology, explanans and explanandum cannot be separated. Or, while the presidents are representatives, their representation is uncertain; Kohn (1987) referred (on rather different grounds) to the 'obvious question of the validity of using the interests of ASA presidents as an index of substantive concerns of U.S. sociology'. Again, the pattern found with 'science' - that Merton wrote in detail on what other presidents treated glancingly - was repeated for most themes: three presidents wrote specifically on 'race', for example, but it was at least a minor issue in twenty-nine other addresses; or ten other presidents referred in more or less detail to the mass media of Wirth's (1947) and Gamson's (1994) explicit attention. Since this 'topical shift' elides disciplinary topics and disciplinary resources, analysis in these terms would be preemptive.

The addresses might be taken as topical in another sense. Wilner (1985), for example, included them in her analysis of all the articles from the *ASR*, in which she concluded that American sociology was not 'relevant' to the major events of the day. This too is problematic, for, on Mannheim's sense of the documentary, the addresses cannot not be 'relevant'. That was shown in the content-analysis, for all presidents used

the 'locational repertoire', and thus evoked a common sense of the here-and-now.

But if those two points of departure are debarred, a third remains. When Wirth (1947) noted that a presidential address was a chance 'to offer a discourse on one's favorite topic' and that his own 'favorite topic' was sociology, he made explicit what others took for granted. Whatever their substantive interests, the presidents are, as Goffman (1982) said, 'representative of something', and that 'something' is sociology itself. This too was confirmed in the content-analysis; over-one third of the presidents used 'sociology' or its derivatives in their titles, all but one used it in their addresses, and usage was high. So on a rhetorical reading of the addresses, the topic of sociology can be treated through 'the topics' used in them.

'The topics', or commonplaces, in classical rhetoric are well suited to a sociology of disciplinary knowledge. Prelli's (1989a: 258) reading of them as "repeatable and acceptable themes that deal with shared beliefs, values, and opinions ... to do with situationally appropriate thoughts and actions" matches Gilbert and Mulkay's (1984) 'repertoires' or Law's (1994) 'modes of ordering'. Fractal ordering of the disciplinary was derived from their sense as contestable sources of arguments. Disciplinary was defined on that basis as comprising *topoi* of 'collectivity', 'science', and 'usefulness', with each of these being a material and embodied clustering of conflictual expectations. That is the sense used in this chapter. It is applied first to the topic of 'collectivity'.

FOLK COLLECTIVITY - I

Any doubt that the addresses are effects of 'collectivity' was removed in the quantitative analyses. Use of the first person plural not only distinguished them from standard articles but emerged as well as the leading element in the leading pattern in the correspondence-analysis. Its link there with 'discipline' confirmed that the addresses are situationally given as means and expression of collective renewal.

That limits what can be said at this point. Just as it was axiomatic in the sociologies of knowledge in Part I that "human knowledge, insofar as man is a 'member' of a society in general, is not empirical but '*a priori*' knowledge" (Scheler 1925: 67), so 'collectivity' pervades every word of both the addresses and the treatment of them in Part III. It emerges from the scientific and useful moments of 'disciplinarity' illustrated in this chapter, and they in turn are contested in and through it; it is the source and

issue of the 'disciplining' in Chapter 8; and it enacts the constantly failing character of 'discipline' in Chapter 9. It is intrinsically reflexive, since study of the formation and maintenance of collectivity is given as the object of sociology. It is, finally, fractal, being evoked isomorphically at any scale; as seen in the case-study of Wirth (1947) and Gamson (1994), for example, 'we-ness' applied, and simultaneously so, to specific co-workers, to members of a disciplinary speciality, to American sociologists, to Americans or sociologists in general, or to a global community. Since these themes emerge in the following sections of this chapter, the collective moment of disciplinarity will be briefly introduced here, and then reprised in the light of the other two topics.

The addresses are suffused with intimations of disciplinary collectivity. The implications of Bendix's (1970) claim that '[l]ike all academic disciplines sociology depends on the existence of a scholarly community' can be found in any number of them. The disciplinary community entailed formal and informal processes, as Sanderson (1942) made clear: '[o]ur annual meetings, and the publication of our official journal ... promote a certain *esprit de corps*, which needs strengthening'. Williams (1958) invoked the analytical commonplaces of his time when he looked to common ground between different interpretations of the discipline (and it might be noted that his presidency fell in the centennial year of Durkheim's birth):

To [different] types of sociology correspond reference groups - the standard setters, comparison groups, aspiration groups, audiences, judges, and gate-keepers of career lines. The collective views of these rather vague and shifting collectivities represent different kinds of consensus as to the norms and goals of sociological study, albeit a consensus marked by a high standard deviation and rather low test-retest reliability. To the extent that these norms are internalized, they may be described as variants of a common sociological conscience.

The affective coherence undergirding that 'vague and shifting' *conscience collectif* is implied in an image of 'disciplinary pride'. Gans (1988), for example, maintained that:

Being useful, as teachers, researchers, writers, practitioners, and as experts, advisers, and critics, will make us feel more useful - and this will strengthen the commonality of purpose among us. Being useful should also add to our pride in the discipline, and pride is itself a potent social cement.

That is enough for the moment to suggest the pervasive implication that sociology is a moral community. Given in Williams' stress on 'science' and in Gans' on 'usefulness', 'collectivity' is inextricably interwoven with the other topics of disciplinarity. As Ellsworth Faris (1937) - one of the most 'disciplinary' presidents in the correspondence-analysis - put the conjunction:

The triumphs of the age of physical science give us confidence in the power of human

reason. The forces of human nature may be used and controlled to satisfy and increase the wants of man. Ours is a profession of the highest dignity. There is every reason to hope that by our efforts human welfare may be advanced.

Faris' 'hope' epitomises disciplinarity. The presidents' optimistic invocations of 'physical science' and 'human welfare' will be shown in the following two sections, before 'collectivity' is reviewed through their conjunction.

FOLK SCIENCE

As with 'collectivity', there can be little doubt that 'science' is a foundational and abiding imaginary of American sociology. At the height of its golden age, for example, Williams {1958} recalled the first presidential address:

Lester F. Ward... began with these words: 'I do not propose on this occasion to enter into any defense of the claims of sociology to be called a science. I wish simply to show that its history, and the steps in its establishment, do not essentially differ from those of other sciences'. Ward went on to argue that the then current state of sociology was comparable to that of astronomy in the seventeenth century or of chemistry 'before the discovery of the true nature of combustion'.

While few of the presidents explicitly defended sociology as a science any more than Ward, all assumed what underlay his 'simply'. That was confirmed quantitatively, for 'science' itself was widely used in the addresses, all presidents used the 'empiricist repertoire', and 'science' was the leading term in the second and third dimensions of the correspondence-analysis. Those two appearances, and the opposite associations with 'discipline' in them, also confirmed the contestability of 'science', for while the presidents might have agreed that sociology was scientific, what that entailed was another matter. Their references to it are presented here in four stages - imaginaries of science, methods of science, lawful science, and contested science - before its topical effectivity is summarised.

Imaginaries of science

The physical and biological sciences appear in the addresses as permanent imaginaries. Given the place in sociology of, say, Comte's 'social physics' or Spencer's 'social evolution', that is hardly surprising, and it was found in the citation-analysis that Newton, Darwin or Einstein could be ranked with specifically disciplinary folk heroes. As imaginaries, the physical and biological sciences served as evolutionary standards and as sources of performative analogies.

The first move has been conventional in sociology ever since Comte coined the word. Hauser's {1968} version might have been disputed in detail, as would the 'social engineering' he justified from it, but his periodisation, his comparison, and his 'hope' were matters of disciplinary common sense:

It was only during the century roughly from about 1750 to 1850 that the physical sciences achieved the respectability and acceptance that paved the way, through engineering, for the transformation of the physical and material world. It was only during the century roughly from 1850 to 1950 that the biomedical sciences achieved a similar status that paved the way, by means of biomedical engineering, for the remarkable increase in longevity and health. It is to be hoped that the century from 1950 to 2050 will be the period during which the social sciences, including sociology, will achieve a level of respectability and acceptance that will pave the way for social engineering.

The analytical structure of sociology is of course pervaded with analogies drawn from physics. When Vance {1944} revisited the conventional distinction between 'statics' and 'dynamics', when Goode {1972} treated 'force' through categories taken 'as crude parallels to different types of physical forces whose analysis yields relationships that parallel one another in some ways', or when other presidents invoked 'equilibrium', they could draw on disciplinary taken-for-grantedness. More generally, the presidents assumed physics as the standard of a real science. That appeared in the passage which Williams {1958} quoted from Ward, and Ellsworth Faris {1937} had used it earlier:

Many among us are moved to write words of sharp reproof when they contemplate the achievements of sociology in comparison with the rich treasures of physics and seem discouraged that we cannot proceed to the speedy erection of a comparable system

It recurred in Parsons' {1949} justification of general theory, when he spoke of that:

ideal state, scientifically speaking, where most actual operational hypotheses of empirical research are directly derived from a general system of theory. ... only in physics has this state been attained in any science. We cannot expect to be anywhere nearly in sight of it.

It remained a disciplinary common sense. Lieberman {1991}, for example, talked of 'our usual model, the physical sciences', and described a critical test of Einstein's theory as 'a wonderful model or ideal that I believe represents the way many of us implicitly think about evidence'.

The balance between standard and analytical source appears reversed in the presidents' invocations of the biological sciences. They took for granted that social life was evolutionary, despite Moore's {1966} claim that the model had been 'some decades in disrepute', or Hawley's {1978} that human evolution was more Lamarckian than Darwinian. They could then adopt biological language directly. Sorokin {1965}, for example, held that the:

properties of an organism as a system that bears in itself the basis of its individuality and perpetuation, of self-directing change and passage through the immanently predetermined phases in its life-career are applicable, with a slight variation to socio-cultural systems. From the moment of their emergence they also bear in themselves the main phases of their life-career, and this life-career consists largely of an unfolding or realization of their potentialities.

Yinger {1977} similarly treated 'the connection between social change and oppositional movements' seen in countercultures as 'genetic mutations, which stand as powerful analogies'. Or, the connection might be taken beyond metaphor, as when Alice Rossi {1983} called for 'an integrated biosocial science', since '[b]iological processes unfold in a cultural context, and are themselves malleable, not stable and inevitable. So too, cultural processes take place within and through the biological organism'. While such a potential opening of the distinction between nature and culture was rare, the glossing of it through organic tropes was not; they were all but universally used.

These general measurements of sociology against the physical and biological sciences, and these general borrowings of analogies from them, were matched by more specific enrolments: those sciences served as models of method.

Methods of science

The presidents assumed or argued that scientific knowledge is methodologically guaranteed knowledge. In the first address, Chapin {1935} spoke of 'the familiar controversies that rage about the topic: What is the proper scientific method in sociology?'. While he defended a version of operationalisation, he also claimed that he would not to attempt 'to resolve a conflict of views which continues to be socially useful, because it forces the two sides to restate and clarify their concepts and theories'. Whether or not clarity ensued, controversy continued. Blalock {1979}, for example, was concerned that 'we seem badly divided into a myriad of theoretical and methodological schools', and Gans {1988} talked of a 'mindlessness [that] is part and parcel of our passion for methodology'. While only Lazarsfeld {1962} devoted his address to methodology as such, the other presidents were tacit methodologists in the same sense that they were tacit sociologists of knowledge. It might have been agreed that development of 'methods' was necessary - as Queen {1941} said, 'the continued growth of our science' demanded it - but their entailments remained uncertain. Many presidents spoke of 'testing hypotheses', but since criteria of success or failure were scarcely mentioned the scientificity of methods appeared a matter of common sense. In

this, images drawn from the physical and biological sciences are prominent.

Sociology, for example, might need the 'experiment' of Lundberg's (1943) claim:

With the disappearance of the notion that social phenomena were divided by God into two categories, those that can be quantified and those which cannot, a vigorous experimental movement ... has appeared in sociology and psychology.

Williams (1958) also welcomed an 'increased use of approximations to experimental design' in the discipline. But in the meantime, Stouffer (1953) had noted that '[o]nly a few sociologists are now trying to do experiments', while Donald Young (1955), looking to strengthen the ties between the discipline and the 'practicing professions', had suggested that sociology had 'relatively limited scope for contrived experimental verification of hypotheses and findings'. Still, if the accounts of the discipline were inconsistent, 'experiment' continued to be used; Peter Rossi (1980), for one, couched the study he reported in terms of it.

The image of 'laboratory' also recurred. In his review of empirical research, for example, Lazarsfeld (1962) cited Albion Small's claim that 'the contemporary sciences - of nature as well as of society - required the laboratory'. Chapin (1935) denied that because they did not have the 'advantageous laboratory situation' of the physical and biological scientists sociologists were barred from the experimental method; variables could be controlled by selection of representative samples. Or Hughes (1963) talked of interracial relations as a 'living laboratory'. The image was not universally endorsed. While Stouffer (1953) and Komarovsky (1973) reported on 'laboratory investigations', Gans (1988) claimed as one of sociology's distinctive and potentially appealing qualities that its findings:

are based on research among ordinary Americans. While other social sciences concentrate on elite decision-makers, exotic subcultures, or laboratory subjects sociology has always done much of its work with and among typical Americans.

For him, 'intensive interviewing and fieldwork are generally more scientific' than work in laboratories. But whatever the disagreement over detail, the assumption of a scientific method remained, and with it the hope of lawfulness.

Lawful science

It is crucial to science as a sociological *topos* that scientific methods ensured objective accounts of social facts. On that basis, for example, Hankins (1938) held that the social sciences were 'the unrelenting enemies of superstitions, sentimentalities, and

dogmatisms in every field of social life', and that they were 'the chief promoters of the realistic and objective thinking that is the basis of all rational action'. To be sure, there were difficulties in fulfilling what Becker (1960) saw as necessary for any sociology, 'remaining in close touch with the empirical evidence, with the "damned facts"'. For Davis (1959), the problems of social study arose 'mainly, if not exclusively, from the circumstances that the observer must analyze objectively the norms of conduct that he and others, as actors, react to emotionally', but the ability to remain detached distinguished the sociologist. This required that disciplinary standards of objectivity not be compromised in the name of tolerance, for as Yinger (1977) warned, 'we need to guard against pluralistic ignorance - a shared belief in pseudo-facts'. Such pseudo-facts might emerge from undeveloped theory as well as from loss of objectivity or poorly chosen methods. That was Parsons' (1949) justification for the suggestion that 'theory is even more important in our field than in the natural sciences'. Once sound theory was in place, a cumulation of knowledge could be expected. Davis (1959), for example, claimed that '[f]rom the standpoint of scientific discovery, the interesting part of theory is not the verified but the unverified propositions. A theory proved is no longer theory; it is fact'. But since facts were there to be gathered, theory might not always be necessary; Lieberman (1991) held that the answers to some questions:

provide useful information about society even if they sometimes are not couched in theoretical terms. Only a discipline trained to tie itself in knots can manage to find something wrong with such an activity and denigrate it - usually with the 'positivist' label. (By the way, who said that knowing some facts is such a bad idea ?)

Once enough facts were gathered, whatever their grounding in theory, sociologists were required to derive laws from them.

Again, the 'laws of nature' of the physical and biological sciences served as the model for sociology. As Sorokin (1965) said, the "'physiological" (repeated processes)' of social life made it amenable to analysis. He did grant that the scale of study of those processes had to be extended - by themselves small-scale 'analytical and fact-finding theories ... foster pseudo-scientific ignorance rather than scientific truth' - but the objective remained. Whether it appeared in terms of uniformity, law, regularity, generalisation, or prediction, the lawfulness of sociology was a recurrent theme. It can be found from Fairchild's (1936) expectation of 'well-authenticated laws' through to Coleman's (1992) plan for his 'bounty on children' to be calculated through 'use of social science methods to make a statistical prediction... of the expected costs and benefits to

government of a given child'. An element of chance might remain, but as Moore {1966} said, 'chance is essentially an expression of ignorance and is therefore subject to curtailment as knowledge increases'. For Blalock {1979}, prediction was one mark of the true scientificity which would allow sociology to be united:

Clearly, we need theories that are sufficiently general to integrate our fragmenting discipline into reasonably coherent bundles. These theories must be precise enough to yield predictions that are both falsifiable and that extend beyond common sense.

Lawfulness could also serve as a disciplinary marker. In defending cross-national study, Kohn {1987} claimed that such analyses were aimed at 'generalizations that transcend particular historical experiences in a search for more general explanatory principles. In short, the method may be historical, the interpretation should be sociological'.

But if the aim of lawful, universal, knowledge was regularly assumed, doubt over its possibility also recurred. Following Chapin's {1935} stress on unexpected consequences, Hankins {1938} claimed that for all their statistically objective study of data social scientists have 'as yet, very little power of prediction', and that '[o]nly one conclusion seems certain, namely, that in the long run we reach a wholly unexpected social condition'. In calling for more sweeping patterns to be recognised, Sorokin {1965} applied physicists' description of sub-atomic particles as the 'microcosm of lawlessness' to the 'unique socio-cultural phenomena' on which many sociologists concentrated. Hughes {1963} took as one point of departure a notable failure in sociological prediction:

Some have asked why we did not foresee the great mass movement of Negroes; it may be that our conception of social science is so empirical, so limited to little bundles of fact applied to little hypotheses, that we are incapable of entertaining a broad range of possibilities, of following out the madly unlikely combinations of social circumstances. ... Why should we have thought, apart from the comfort of it, that the relations of the future could be predicted in terms of moderate trends, rather than by the model of the slow burn reaching the heat of massive explosion?

When Lipset {1993} granted that the 'record of social scientists as futurologists is not good', he suggested less psychological limits to prediction: '[g]iven the multivariate nature of whatever causal nexus is suggested, it is inevitable that any given variable or policy will be associated with contradictory outcomes'.

While Lipset did not elaborate the point, his assumption of the limits, and even contradictoriness, of 'science' was, if not an orthodoxy, at least a recognised disciplinary stance. Other presidents treated science more as contested than as given.

Contested science

While no president appeared to question that sociology must be a science, doubts did appear over both the meaning and the sufficiency of 'science'. Since it is elaborated in the revisiting of 'collectivity', this questioning of science is illustrated here through only two passages. Williams' (1958) version of the distinction between nomothetic and ideographic inquiry was written from the heart of the structural functionalism associated with disciplinary scientificity:

there is no reason to suppose that sociology of the future will be exactly the same as that of the present, nor does it have to be fashioned according to the models of physical and biological sciences. The most tenable definition of 'science', in any case, is a range definition based on the history of the very diverse special sciences. Whether we approve or disapprove of it, it is a fact that sociology today contains both the scientific aspiration to derive analytical laws explaining relations among precisely indexed abstract variables, and the historical intent to communicate a part of the experiential richness of concrete human action. It may be predicted, without too much daring, that for the foreseeable future both emphases will be with us.

Bendix (1970) echoed him, but more sombrely. Describing sociology as 'a discipline whose scientific aspirations are well ahead of its achievements', and claiming that 'sociologists as scientists face a crisis of legitimacy' at a time pervaded by an 'ethic of despair', he said:

To me the tensions and debates between the scientific and the humanistic impulse appear as the foundation of modern sociology. Twenty years ago I wrote an essay on social science and the distrust of reason. My purpose then was to contrast an unreflective faith in science with the tradition of critical self-scrutiny reaching from Francis Bacon to Sigmund Freud. I wanted to warn that methodological preoccupations not be permitted to encroach on substantive concerns, lest we do harm to our discipline. ... today I would emphasize that the distrust of reason is not furthered by scientism alone. It consists also in a consciousness of crisis, an ethic of despair, and a call for action which do away with learning and deliberation altogether. I think sociology is as endangered by this retreat from meaning and coherence as it was by spurious analogies from the natural sciences.

Since Bendix's doubts are matched in other addresses, it might appear misleading to treat 'science' as 'optimistic'. But his very intensity suggests a hope betrayed, and the

point remains that 'science' was a focus for it. The fact that it was worth disputing makes it a disciplinary topic.

Folk science: Reprise

The presidents widely assumed the scientificity of sociology. The physical and biological sciences serve as performative imaginaries of disciplinary development, disciplinary methods, and disciplinary lawfulness. It is also clear that no unitary

reading of 'science' is possible. While no president questioned science as such, each version of it, each point, each image, was debatable. On both counts, 'science' is a topic of disciplinarity. It has the inherent contestability expected of rhetorical *topoi*, it is available for situational deployment, and it is used optimistically. Amid the substantive topics of the addresses, the generally unexamined assumption of sociology's scientificity stood as one component of the reflexivity expected of the presidents. A similar result is reached from examination of 'usefulness'.

FOLK USEFULNESS

That sociology is not only a science but a useful science is also foundational to a discipline derived in part from religious and secular movements for social reform. Now at first glance, this topic appears to be less salient in the addresses than the scientific, for while the 'repertoire of usefulness' was found to be universally used, it did not emerge as focal in the correspondence-analysis in the way that 'science' did. It was suggested then that this absence may have been either an artefact of the analysis or an effect of taken-for-grantedness. The latter is supported on the reading here, since ameliorative engagement with the world beyond appears to have had a recognised if contestable place in the discipline. Hughes (1963), for example, stressed it in his imagining of 'a grand and flexible division of labour among us':

Some of us bend our efforts toward making sociology immediately useful to people who carry on action or who have problems to solve; I would hope that breed would serve the impecunious and deviant as well as the well-heeled and legitimate, those who seek radical solutions to problems of society as well as those who want merely to maintain stability. Others of us make models of societies, large and small, without much thought as to whether societies corresponding to the models exist at present. Let them be even more free in their imagination than they are. Let those who perform experiments go ahead ... Finally, there are among us some who look about the world for laboratory cases in which to study the problems of human society; and those who, deeply and passionately involved in some problem of real life, describe reality both with the intimacy and detail which comes from close participation and observation and with that utopian imagination which can conceive of all sorts of alternatives to the way things are now ...

Although not all presidents spoke directly of the betterment which Hughes imagined, only Davis (1959) explicitly distanced sociology from it: '[t]o applied sociologists functional analysis is uncomfortable, to social reformers it is anathema, because it subjects to scrutiny the very goals for which application is made or reform intended'. Another two implied dissent. Sutherland (1939) depicted his study of white-collar crime as being 'for the purpose of developing the theories of criminal behavior, not for

the purpose of muckraking or of reforming anything except criminology', and Goffman {1982} saw sociology's strength in 'the bent to sustain in regard to all elements of social life a spirit of unfettered, unsponsored inquiry, and the wisdom not to look elsewhere but ourselves and our discipline for this mandate'. But Sutherland's very disclaimer points to the salience of 'reform', and Goffman's 'unsponsored' implies an ameliorative bent. The presidents may then have widely assumed that sociologists at least could, and perhaps should, be concerned with betterment, but this assumption was as contestably developed as was 'science'. It does entail a reversed emphasis: while the presidents imagined scientific methods as ensuring the lawfulness of sociological knowledge, they took that lawfulness to warrant the methods of usefulness.

Imaginariness of usefulness

The presidents' assumption that sociological knowledge was useful knowledge was particularly clear in the period before the golden age. Speaking of Roosevelt's administration, Fairchild {1936} regretted that while many economists were included, 'an almost negligible part of the responsibility was entrusted to those who had made sociology their life study and work'. Attention to 'business as an institution' would redress this imbalance:

For it is indubitably evident that a large portion of the anomalies and futilities of the contemporary social structure are essentially economic in their character, and that only as sociological interpretations and techniques are made available can the faults in either structure or function be corrected...

World War II and its aftermath gave scope for this imaginary. Sanderson {1942} found it 'obvious' that 'sociological research and teaching are essential for giving men an understanding of the social forces involved in the democratizing of society', and Lundberg {1943} too looked to disciplinary taken-for-grantedness:

I assume it is self-evident that a desirable peace settlement involves primarily a knowledge of sociological subject matter. All sociologists, at least, will presumably agree. We believe that knowledge of the type which sociologists possess, or aspire to, is an essential and a technical requirement for a satisfactory peace just as the knowledge that physical scientists possess is necessary for modern war.

A similar sense is evident in Znaniecki's {1954} insistence that a set of:

sociological problems ... must be solved, not only for the advancement of sociology as a science, but also to enable social leaders to collaborate in planning and promoting cultural creativeness and the social integration of humanity in order to prevent cultural destruction and social disintegration.

During the golden age, sociology's usefulness was not defended with the intensity of

those extracts. When Donald Young {1955} called for greater engagement with the 'practicing professions', on the grounds that the 'community from which sociologists draw their livelihood expects them to earn their keep; and the ultimate judge of whether they ... are justifying their existence is society, not the profession itself', he made only limited claims for the discipline. Lazarsfeld {1962} was just as sceptical:

Having grown up in an exciting and constructive period of socialist optimism, I have never quite lost my hope for radical social change. But I do not believe that empirical social research of the type we are discussing tonight can contribute much to it.

But as in Hughes' {1963} 'division of labour', the imaginary continued. Moore {1966} treated it almost as a suppressed tradition, in his address on 'the utility of utopias':

It would be improper as well as useless to suggest that every sociologist drop his current intellectual concerns to devote his talents exclusively to building a better world. Even if that were the sole acknowledged rationale for our existence as a discipline, which is dubious, we are not well equipped with knowledge as to just how a better world would look, or how it might be brought about. Some of our current careful work is bound to be useful in constructing and reconstructing utopias, even if we have no such lofty ambitions or reject them as professionally improper. My only plea is for indulgence toward our brethren who think that man is worth saving and his lot in life worth improving.

Thereafter, it appeared more often: in Hauser's {1968} claim, for example, that '[i]t is the role of the social sciences, in general, as well as sociology in particular, to provide the necessary knowledge' for social engineering; in Coser's {1975} taking it for granted that the discipline 'will be judged in the last analysis on the basis of the substantive enlightenment which it is able to supply about the social structures in which we are enmeshed'; or in Peter Rossi's {1980} noting that applied social research attracted 'those of us who are still concerned with improving our society'. It might even be held that at least some of the ambitions of the early presidents had been fulfilled, as in Whyte's {1981} introduction:

...In President Reagan's initial budget proposal for 1982, we were told, in effect, that what we (and other social scientists) do in research is of little relevance in solving national problems. On past performance, we do not warrant that judgment, but let us not expend our energies in defending past accomplishments. We must do better in the future to demonstrate the practical relevance of sociology.

Other presidents too evoked the early years. Claiming that in the study of risk the 'limitations of analyses based solely on engineering, biology, systems analysis, even economic or psychological criteria and assumptions, are apparent', Short {1984} held that 'the domain, if not the knowledge, analyses, and insights of sociologists and other social sciences are recognized as critical'. Or Coleman {1992} showed all the intensity and urgency of the wartime presidents in his call for sociological engagement in the

'rational reconstruction of society'.

So while it may have been partially eclipsed during the golden age, usefulness appears abidingly constitutive of 'the discipline'. Furthermore, and as suggested by Coleman's use of 'rational', if sociology is useful it is because it is lawful.

Lawful usefulness

It was shown under 'folk science' that the presidents treated scientifically warranted knowledge as lawful, and now it will be seen that they treat lawful knowledge as applicable. As Ellsworth Faris {1937} put that conjunction:

It is for us by hard thinking and disinterested search, by co-operative effort, to dig and to discover, to understand the causes and conditions - for only on intelligent comprehension of facts, principles, causes and conditions, can intelligent programs of action be based.

While Bendix {1970} suggested that '[o]rdinarily we do not think of science and scholarship as bases of authority', that was precisely what the presidents concerned with usefulness assumed; the lawfulness of sociological findings gave them authority. Calling for greater international cooperation, for example, Angell {1951} held that:

Since interaction among nations has been going on for centuries, one can assume that the ultimately successful links or bridges between them already exist in some embryonic form. What is needed is an intelligent process of selection so as to maintain and multiply the links that are holding fast and to eliminate those that cannot support the burden. The role of the sociologist, then, is to speed up a process that would involve us in needless travail if left to crude trial and error.
... the life process itself makes the necessary social inventions. It is for sociological research to determine which are fit to survive.

Robert Faris {1961} presented a similar sense of a 'life process' needing lawful direction, in his address on the fostering of talent:

attractive potentialities of increase in collective ability are possible if we advance our knowledge of the sociological influences that stimulate and limit aspiration and achievement, and find strategic points at which we may establish some control over them. No great difficulties appear to stand in the way. It appears that we only need to apply a massive research effort in the field of the relation of social factors to abilities. Fortunately there exists today a nation-wide enthusiasm for the development of talent resources; a milling crowd is stirring into action even in advance of academic sociological leadership.

Coleman {1992} tacitly repeated that 'even' when he held that the 'construction of society will go on with or without sociologists', and that 'the ultimate justification of all [sociological theory and findings] will be their contribution to optimal design of the constructed social organization of the future'. The imagining of lawful authority is clear: in Angell's 'determine which are fit to survive', in Faris' 'establish some control', and in Coleman's 'construction of society'.

To be sure, that hope was contested, as when Gans {1988} suggested that 'the search for sociological 'laws' ... may express the latent hope for power - in an ideal society - in which these laws and their formulators would play a central decision-making role', or when Peter Rossi held that:

Even at its best, applied social research does not substitute for the political process. It merely provides another input into the policy-making process. Indeed, who would have it otherwise? One of the virtues of our political system is that decisions are made often enough as the outcome of the pulling and hauling among a variety of interest groups, a process that may value the input of social science work but does not place the work on a pedestal of absolute authority.

But presidents sceptical of either the potential or the state of sociological knowledge might still imagine a rational lawfulness. Amid doubts over 'fully rational action', for example, Hankins {1938} contrasted the way that '[t]he man on the street is not entitled to an opinion in questions of physics or chemistry, medicine, or psychiatry' with the state of the social sciences: '[h]owever ignorant he may be of the theoretical aspects of a social problem, the average man feels fully entitled to an opinion'. Lieberman {1991} similarly imagined an authority beyond opinion:

We often provide strong conclusions based on evidence consistent with those conclusions, but for which the evidence is not sufficiently strong to rule out reasonable alternative causal interpretations. The larger society is therefore free to follow its biases.

The usefulness of sociological knowledge then hinged on the lawful elimination of bias. That same sense recurred in the presidents' assumptions or remarks on the methods by which sociological usefulness might be realised.

Methods of usefulness

If sociologists were to achieve their imagined directive role, then they had to be involved in policy-making. The difficulties in that are summed up in the peculiarly self-exemplifying address of the first president in this study. Having talked on the one hand of unexpected consequences and on the other of the need for sociological theory to be operationalised, Chapin {1935} concluded that the:

first application of sound social theory is to examine critically all utopian ideologies that underlie planned social action. The second application is to forecast consequences arising out of the unplanned combinations of those social actions that are independently planned.

His proposal for application - and if his conflation of ideology and utopia was non-Mannheimian it nonetheless foreshadowed Mannheim's own trajectory - entailed a place for sociologists at the heart of government:

All projected legislation designed to achieve social and economic objectives by initiating changes in the existing structure of social organization should be referred to ... planning boards for comment. There should be attached to such boards, as technical consultants, persons who are trained social scientists. Such persons should be actually advisory in the sense of being consulted before and not after administrative decision is made. They should render an expert opinion by stating the alternatives of social action and/or the probable consequences, and submit this statement to the proper public official or leader. Finally, this expert opinion should be considered and studied by administrators before the decision is announced publicly.

No other president advanced so specific a mechanism for making sociology useful.

Instead, even those committed to betterment appeared to doubt that sociology had a place in policy-making. Just as Lazarsfeld {1962} queried the usefulness to reform of his empirical research, Kimball Young {1945} stressed the limits of sociological knowledge:

It is all very well and easy for some of our colleagues to argue that if we only had scientific findings the answers in policy would be self-evident. Granted this counsel of perfection, we seldom possess sufficient pertinent facts and in the meantime human choices or decisions of program-making must be made.

When findings were 'scientific', they might also be discouraging. That was Sewell's {1971} experience:

when we look at the components of socioeconomic status we find that no one of them plays any unique part in the causal system explaining attainment in higher education. This is unfortunate from the standpoint of policy considerations. One would have wished that family income might have had a larger and a more special set of effects because it is the aspect of socioeconomic background most readily amenable to change.

Realpolitik was just as stubborn a social fact. Hankins {1938}, for example, claimed that 'social scientists are ... widely distrusted', and that a politician 'may use them, from time to time, as a "brain trust" only to cast them aside when their views or their factual knowledge no longer serve his political objectives', and Peter Rossi {1980} made a similar point:

The applied researcher ordinarily does not get very close to the seats of decision making and policy formation. Often enough, policy appears impermeable to both the results of research and the advice of the researcher.

But if policy-making allowed limited scope for sociological usefulness, other methods remained. Donald Young {1955} suggested one, in looking to greater cooperation between sociologists and the 'practicing professions'. Another arises in Taylor's {1946} call for sociologists to participate more in practical 'action'; that would allow them to 'convert a considerable body of social science into common sense knowledge by making it part of the working knowledge of those responsible for practical programs of social action'. Rossi's stress on the limited permeability of policy and Taylor's intimation of a quasi-hermeneutic circling between science and common sense point to what other presidents

more generally gloss in their simultaneous assumptions of usefulness and bypassing of specific mechanisms for its realisation: methods for the application of sociological knowledge belie the lawfulness ostensibly justifying them. That point, too, appeared in the addresses. Just as pervasively imagined as 'science', 'usefulness' is also as contested.

Contested usefulness

Useful knowledge is political knowledge, and whether the presidents accepted the link or distanced the discipline, political knowledge is by definition contestable. It was noted that only Davis (1959) explicitly denied 'usefulness', when he took Weberian *Wertfreiheit* for granted in identifying functionalism with sociology. But even if that were the 'ruling myth' which Gouldner (1961) suggested, it was neither universally accepted - rather, it was disciplinary common sense that sociological knowledge could or should be useful - nor sustained. Gans (1988), for example, welcomed the way that applied sociology had become part of the discipline 'once we were able to move beyond the primitive conceptions of value-free sociology on which the early disapproval of social policy research was based'. The presidents might even be openly partisan, as Gamson (1994, emphasis added) was in his address on 'the politics of exclusion':

My intent, *of course*, is not simply to gain a better understanding of processes of exclusion, but also to suggest ways to prevent active exclusion and to challenge indirect exclusion. Like any sociological analysis with an action or policy agenda, I implicitly address the issues from the perspective of social actors who can influence social policy. My approach differs, however, in the particular actor perspective employed. Policy analysis, including the sociological variety, often involves an implicit managerial perspective. The assumed social actors addressed are state and corporate managers who have the ability to make decisions that collectively produce public policy on the issue being analyzed. In contrast, the assumed social actors I have addressed in this paper are the targets of exclusion and the sympathetic third parties who could mobilize to intervene on their behalf.

Gamson's 'implicit managerial perspective' is well supported by the passages on the lawfulness of sociology; sociological knowledge is embedded in more general political processes. Even if detachment were possible, it was 'notorious that social scientists do not ordinarily agree on the best policy to adopt in a given situation' (Hankins 1938). That might even be part of professional careers, as sociologists became mercenaries of disagreement; Peter Rossi (1980) noted that the application of disciplinary knowledge 'has spawned part time employment of researchers as methodological critics hired by partisans to provide devastating criticisms of some applied social research'.

At that point, the contestability of sociological usefulness is co-extensive with

politics more generally.

Folk usefulness: Reprise

This section was intended to show that sociology's usefulness is as pervasive in the presidents' representations of 'the discipline' as is its scientificity. The range of presidents quoted, across time and across disciplinary affiliations, is enough to demonstrate that. The usefulness of disciplinary knowledge was seen in both direct statements and assumptions, and was at issue in both the lawfulness and the methods either implied or given in the presidents' articulating of the discipline to the world at large. But as with 'science', each version of 'usefulness', each point, each image, was open to question. Through its recurrence and constestability, and through its generally optimistic deployment, usefulness too is a topic of disciplinarity. It is also evident, first, that those two moments of disciplinarity are interpenetrative - usefulness can only artificially be separated from science in all the passages above - and then that that interaction implies the third, to which attention is now returned: 'collectivity'.

FOLK COLLECTIVITY - II

In the earlier section on 'folk collectivity', 'the discipline' was depicted as a moral community, and in the two previous sections as being scientific and useful. Since these in turn are interactive - '[f]undamental research is sterile, its value is unproven, without its application' (Sanderson 1942); 'only on intelligent comprehension of facts, principles, causes and conditions, can intelligent programs of action be based' (Ellsworth Faris 1937) - the disciplinarity in the addresses is consistent with the trinitarian modelling in Part I. That was derived to replace the dualistic separation of political from epistemological senses of representation; knowledge which is epistemologically scientific and politically useful is also collectively lived. Now that the first two moments have been read against the context of the third, it in turn is revisited in the light of their interaction. Once again, treatment is brief, since collectivity will be at issue through the next two chapters. It is enough to show now that 'folk collectivity' is itself a fractal of collectivity, science, and usefulness: it is an effect of an emergent tradition, it entails purifying reference to the distinction of the scientific from the political, and it involves a pragmatic grasping of opportunity which belies that

distinction. In each of these and in the overall effect, the discipline's collectivity is as uncertain as its scientificity and its usefulness.

Tradition

American sociology was formalised from an imaginary of disciplined direction of social betterment. Moore {1966} invoked that, when he tentatively breached the orthodox consensus on scientificity of the golden age by recalling that '[t]he older intellectual traditions of sociology favored the use of current knowledge for remodeling as well as merely describing the social order'. For all that Peter Rossi {1980} was to warn that 'applied social research is no occupation for would be philosopher kings', he could still point to the permanence of that disciplinary vision:

our roots in applied concerns are old and very much alive. Our ranks have always been full of ministers and ex-ministers, radicals and ex-radicals, even a few conservatives and ex-conservatives, all of whom were attracted to sociology because our discipline appeared to have some relevance to social reform or its prevention.

But, of course, it was not restricted to the discipline. When Ellsworth Faris {1937} referred to 'our public whose interest and permission is essential if we are to have support for our work', or when Gans {1988} held that if 'members of the lay public feel that our work is useful or enlightening or both, they have an incentive to give us their cultural and political support if we need it', they took for granted that it was more widely shared. Donald Young {1955} made that assumption of embeddedness explicit:

The expectation of usefulness dominates the popular view of science in the United States in spite of apparently increasing respect for those who contribute to the store of knowledge of man and nature without immediate concern for utility or reward. That sense of sociology being a particular tradition within a more general version of it was evident in the sections on 'science' and 'usefulness', and the distinctiveness of the sociological collectivity is then uncertain. Folk resolution of the problem entailed versions of the 'work of purification' found in the formal sociologies of knowledge.

Purity and danger

Given sociology's location in a tradition, its distinctiveness entails a separation of the epistemologically scientific from the politically useful. This purification is found in the addresses, and, often enough, in terms of the purity and danger suggesting a collectivity under defence. Donald Young {1955}, for example, warned of a 'danger that unbalanced involvement with social improvement may warp research design, weaken

devotion and adherence to scientific standards, and lead to neglect of fundamental questions'. Hauser {1968} used stronger language in his apologia for social engineering:

I am aware that I have tread perilously on the border between social science and social engineering. I may be accused of polluting the science of sociology with the stigmata of social policy and implied, if not explicit, proposals for social action. I am sensitive, as well as open, to such criticism because I firmly believe in maintaining a sharp boundary between science and engineering.

That Hauser's 'sharp boundary' is a disciplinary common sense is suggested in Sewell's {1971} peroration to his report on path-analytic research on access to higher education:

Americans of all political persuasions have expressed the view that equality of educational opportunity is an essential prerequisite for a well functioning, democratic society. The programs I have discussed detail some measures necessary to begin to implement this essential need. I urge you as citizens to join me in working for their implementation and as sociologists to join me in pursuing further research which will more clearly specify the most effective alternative programs.

Or, instead of a distinction between 'sociologists' and 'citizens', the line might be drawn between 'scientists' and 'professionals'. That was Lee's {1976} point when he held that 'individuals and groups put sociologies together representatively':

Can sociologists - as so many of us claim - have it both ways ? Can we be scientists, and thus reasonably dependable sources for accurate data and for useful theories at the same time that we are professional careerists organizing to raise our statuses and incomes in the commercial scramble within society as it exists today ?

So whether it was treated through contrasts between the scientific and the political or between the scientific and the professional, the collectivity of sociology entailed problematic boundaries. But the collectivity also depended on a pragmatic grasping of opportunities which required their breaching.

Opportunity

When Sanderson {1942} referred to 'this opportunity and responsibility facing us' in his address on sociology and democracy, he made explicit a recurrent feature of the optimism of disciplinarity: social problems are sociological opportunities. As MacIver {1940} said, a 'time of crisis reveals the unseized opportunities. But I present them as opportunities, as auguries of advance and not as witnesses of failure'. Such opportunities were multi-directional. In reporting his and his colleagues' work on 'general theory', Parsons {1949} held that 'a great opportunity exists. ... Can American sociology seize this opportunity?'. Peter Rossi {1980} looked more to lost chances: through the relative neglect of applied research, 'it is clear that we are missing out on some important opportunities. Primary among these missed opportunities are those

that relate to the intellectual health of our discipline'. Rossi's fusion of science and usefulness had precedents. Donald Young {1955}, for example, had welcomed the way that 'many leaders in [the practicing] professions are actively seeking the collaboration of social scientists ... There is indeed little likelihood that sociologists will ignore this opportunity'. Or Robert Faris {1961} had held that '[r]esearch in the sociology of collective ability ... promises to give us an unmatched opportunity to apply advanced techniques of discovery to a matter of critical human importance'. But 'opportunity' was also more pragmatic. For all his criticism of the 'commercial scramble', Lee {1976} included among the aims of his presidency an intention 'to reemphasize the priority of curiosity and of human service in the social roles of sociologists and, thus, to broaden our occupational opportunities'. Peter Rossi {1980}, again, made a similar point in his defence of training in applied research: '[t]here are jobs out there in the applied social research 'industry' that properly trained new sociology Ph.D.s could enter'.

Once again, the standing of the sociological collectivity is uncertain. It may be distinguishable by its members having been 'properly trained', but the pragmatic grasping of the theoretical and practical opportunities opened by social problems calls distinctiveness into question.

Folk collectivity: Reprise

In the optimism and instability of its location within popular expectations over 'science' and 'usefulness', of its defence through images of purity and danger, and of its pragmatic adaptation to opportunities, the 'collectivity' of sociology is as fractally topical as the other two moments of disciplinarity. Nor can it be separated from them; the trinitarian interpenetration of the three, evident in all the passages quoted in this section, makes each of them impossible to treat in isolation. As one of the presidents who most examined the collective resolution of pure and applied sociologies - and, consistently with the modelling of the disciplinary, he was found in the content-analysis to be among the leading users of 'discipline' - Peter Rossi {1980} has been quoted often throughout this chapter. Amid his own insistence on the rigour of 'science', his acceptance of the political limits on 'usefulness', and his calls for a renewal of 'collectivity', he suggested the means by which sociologists in general, and, for purposes here, the presidents in particular, achieved the required reconciliation:

when it comes to facing outward toward the public - and especially toward our benefactors - we are very quick to point to the many complicated social problems that justify

our existence and need our support. Our public stance is often enough that a properly supported sociology will point the way to a better society with a lowered level of social troubles.

That is the strain which recurs throughout the presidents' depictions of the 'we' and the 'our'. Even if he did not use the term 'disciplinary rhetorics', then, Rossi implied their constitutivity. That is the effect being attributed to folk disciplinarity.

FOLK DISCIPLINARITY: REPRISE

Presidential addresses have been shown in this chapter to entail the optimistic imaginary of collectivity, science, and usefulness definitive of 'disciplinarity'. This is a performative epitome of the relatively natural disciplinary worldview, or a fractal of sociology's implication in the processes studied within it. Each moment in it is itself a rhetorical topic, defined more by contestable potential than by specific meanings. All that has been found in the addresses. The presidents took disciplinarity for granted, and in way which was necessarily a folk version of it.

Intimations or evocations of each of collectivity, science, and usefulness pervaded the addresses. Implicitly through using the first person plural, explicitly through images of 'community', the presidents took 'collectivity' as a topic. They of course treated sociology as a 'science', in that the discipline was modelled on the physical and natural sciences as sources of methodologically warranted and lawful knowledge. They just as routinely assumed the 'usefulness' of sociology. In all this, 'collectivity', 'science' and 'usefulness' could not be sharply separated. Since the presidents both presented sociological knowledge as authoritatively applicable because it was scientific and held usefulness as a test of true scientificity, these two moments of disciplinarity were as interpenetrative as each was always already suffused with the third. This means that while the salience of the three *topoi* was unmistakable, the possibility of any president assembling them coherently was another matter. Already contestable in itself, each moment potentially contradicted the others: 'science' and 'usefulness' implied distance and engagement; a 'science' based on methods was more individual than 'collective'; authoritative 'usefulness' required an impossible agreement on the part of the 'collectivity'. Strictly logical articulation of the three would present formidable difficulties. But, as rhetorically occasioned, the addresses are open to the rhetorical resolution given in 'disciplinarity'. Individually

and jointly, the three topical moments might be zones of disagreement, but those zones are agreed. This is what makes the addresses a folk sociology of knowledge.

It was shown in Chapter 5 that despite their occasioned requirement to examine disciplinary knowledge the presidents made scant reference to either the sociologies or philosophies of science and knowledge. But as seen in this chapter, they still fulfilled that obligation. Two strands of scientificity are interwoven in their statements, asides, and assumptions. The first is to be expected from the history of American sociology; whether drawn from Bridgman's operationalism, from one or another version of logical positivism, or from Popper's hypothetico-deductive model, it reflects the philosophies of science of the day. But while the assumptions of facticity, lawfulness and cumulation found in the addresses rest on the sense of methodologically guaranteed objectivity given in them, they were rarely elaborated, and for good rhetorical reasons. Through the situated requirement that they appeal to what was held in common, the presidents were constrained from insisting on any one version of science. Given the need for 'the discipline' to be ceremonially renewed, formal examination of disciplinary knowledge would have been inappropriate, and the presidents then faced a reflexive requirement to blunt their required reflexivity. But 'collectivity', 'science' and 'usefulness' were still salient, and the presidents had available an alternative resolution of them. That was the grounding of their folk disciplinarity.

For an older version of 'science', one which implies both 'collectivity' and 'usefulness' and which appears to be disciplinary common sense, is interwoven in the addresses with contemporary understandings. Topical sociology is Baconian sociology. That imaginary both comprises the three moments of disciplinarity and locates the discipline in more general processes. As Ellsworth Faris (1937) put it:

the account of the transition to the modern age and the story of the discovery of the scientific method has been often told and is familiar to all. But the importance of the discovery can never be overstated. For something over three hundred years ago, a very recent date in the long story of man, there appeared in western Europe a momentous formulation which some have considered as important an event in the life of the race as anything that ever happened. It was this conviction: That the Forces of Nature Can be Used and Controlled to Satisfy and Increase the Wants of Man.

Whether or not Bacon was named - and he was found in the citation-analysis to be the philosopher of science most invoked in the addresses - his vision of science appeared to be assumed. He was tacitly invoked, for example, when Queen (1941) held that 'we have only recently begun to escape from the realm of speculative argument into the realm of inductive reasoning based on concrete data', when Parsons (1949) justified his

general theory on the grounds that 'it is in proportion as sociology attains stature as a science, with a highly generalized and integrated body of fundamental knowledge, that practical usefulness far beyond the present levels will become possible', or when Donald Young (1955) alluded to 'the advancement and utilization of knowledge'. It is evident from the passages quoted throughout this chapter that 'the discipline' was recurrently cast in terms of this Baconian imaginary. As Bendix (1970) said:

The belief in science has remained remarkably consistent from the time of its first articulation in the 17th century to our own day. Francis Bacon wanted to inspire men with confidence that knowledge enhances human power.

However divided 'the discipline' may be, and so however resistant to examination which is both formal and ceremonial, it is suffused by the 'remarkably consistent' collectivity, science, and usefulness of that Baconian common sense. That is its folk disciplinarity. Sociology may be a house of many mansions, but it is a Salomon's House. It is depicted as a source of authoritative knowledge to be authoritatively applied by an authoritative collectivity. Renewed through Comte and Ward and any number of other writers from the earliest days of sociology to the present, that imaginary shows the implication of 'the discipline' in all the other forms of authority studied within it.

That completes the first step of the demonstration that the presidents are folk sociologists of disciplinary knowledge. Their folk versions of collectivity, science, and usefulness show the same legislative strains as in the disciplinarity modelled from the formal sociologies of knowledge. After the relation of 'disciplinarity' to the 'governmentality' from which it was in part derived is illustrated in Taylor's (1946) address, the next chapter is devoted to the second stage of the model, 'disciplining'.

CASE 7: TAYLOR'S DISCIPLINARITY

Taylor (1946) is at once a singular and a characteristic president. No other depicted disciplinary knowledge as an effect of collectivity, science, and usefulness more sharply, and both his address and his career epitomise too the interfusion of the governmental and the disciplinary in American sociology. 'Disciplinarity' was derived from the continuity between Bacon's (1620: 383) claim that "[k]nowledge and human power are synonymous" and Foucault's (1979) subsumption of 'power/knowledge' in 'governmentality'; this last denoted an historical shift to institutions, procedures, and analyses targeting populations, and a tendency toward a 'government' becoming more salient through specific state apparatuses and particular forms of knowledge. The relation of 'disciplinarity' to 'discipline' was set by analogy with Miller and Rose's (1990) linking of 'governmentality' as 'eternally optimistic' to 'government' as 'congenitally failing'. Having been elected while a civil servant, and being even more representative of those who were "fundamentally agents of the state" (Hooks 1983: 389) as a rural sociologist, Taylor exemplifies both governmentality and government, both disciplinarity and discipline. Since he emerged as the leading user of 'knowledge' in the content-analysis, and as a leading figure in the linking of science, discipline and knowledge in the correspondence-analysis, he epitomises as well the standing of the presidents as folk sociologists of knowledge.

Although remembered in rural sociology as a "wise and visionary leader" (Capener 1975: 400), Taylor is among the more obscure former presidents of the ASS/A. Reference to his work in general texts is scant, Mullins (1973) did not list him as an 'important sociologist', and in Westie's (1973) analysis of 'disciplinary survival', only ten of the sixty-three presidents up to 1972 were ranked below him, all of whom preceded him in office. As both a rural sociologist and a Federal civil servant, he was a member of overlapping minorities within the ASS.¹ In 1950, for example, only 8% of ASS members claimed rural sociology as a speciality, while 5% (against 75% in the academy) worked for the Federal Government (Riley 1960: 924, 921). Other rural sociologists became president of the ASS/A and other presidents had held governmental positions, but Taylor was the only one of the sixty under study to have

¹ cf. Taylor's (1946a: 14) obituary of Dwight Sanderson: that he "is not more widely recognized is probably due to [his having] worked in the field of rural sociology with which the so-called 'high-power' sociologists are not too familiar".

been elected during a civil service career. Yet if he was among a minority then and is little remembered now, he was the only second vice-president to have served twice, and he was elected in successive years to the second and first vice-presidencies and to the presidency. This was a rare peaking of honour. After his argument has been sketched, Taylor's eminence will be read as an effect of governmentality/disciplinarity, his later obscurity as indicating the contingency of government/discipline, and Taylor as then being characteristic by his very singularity.²

Taking 'Sociology and common sense' as his theme, Taylor claimed that:

if sociology is to develop into a useful discipline it must combine the type of knowledge and understanding which is derived by use of the most rigid technique of science and by the type of knowledge that is known among practical men as common sense (*sic*).

Since 'common sense' included 'folk knowledge', but referred more particularly to the skills of engineers, administrators, or reporters, 'science', 'usefulness', and an open-ended 'collectivity' thus characterised his vision of sociology. While he granted that the theory and generalisations of pure science were necessary for the discipline he held that they should be developed in the 'laboratories' for the study of human relations to be found through participation in the solutions to practical needs. Rather than being unsatisfactory compromises, the resulting fusions of science and common sense were:

creative in that they amplify so-called theoretical knowledge, validate theories if they are correct and modify them if they are incorrect. It would be difficult to envision the magnitude, progress, and usefulness of sociology if it would systematically and diligently follow this path of development.

This has all the topical optimism of disciplinarity. Taylor assumed it as the basis of both his prescriptions for the external relations of the discipline and his diagnoses of its internal problems. Thus the frequent conflicts in sociology between quantitative and qualitative workers were as foolish as the distance between 'men of science' and 'men of common sense', since both statistical methods and cultural analysis were necessary 'if anything approaching complete analysis is to be made of most social situations'.

Sociologists should include 'participant observers' (local lay experts) in all research, and should also cooperate with such other social scientists as psychologists, economists, statisticians, and anthropologists. While this can be read as a standard appeal for both explanation and understanding in sociology, Taylor's intent was wider. On the

quasi-hermeneutic assumption that common sense was as influenced by science (however

² He was typical, too, in his biographical fit with Mills' (1943) description of 'social pathologists' as mid-Western, rural, Protestant and progressivist (cf. Odum 1951; Falk and Gilbert 1985).

slowly) as much as science should be by common sense,³ he held that training in the discipline was inadequate:

I have for a long time worried about the fact that it takes young sociologists from five to ten years to recover from what happens to them in their graduate training. ... They should be taught that there is no such thing as individual social discovery; that sociology is the study of the behavior of persons in relation to each other and that everything they can ever know about this behavior and these relationships is already known in some of its aspects by those who are involved in them.

Against that background, Taylor offered the results of his own Division within the Department of Agriculture as exemplary of what was achievable more generally were sociologists to abandon their 'semi-esoteric ways of life. Disciplinary knowledge had been advanced, while 'administrators of agricultural and rural life programs, leaders of rural people, and farm people themselves' had been assisted in 'the social, economic, and even technical and physical issues of living and making a living by farming'.

If Taylor's imaginary of a 'sound discipline' is the expected fusion of science, collectivity and usefulness, his account of the discipline as practised also shows the expected fuzziness and 'failure'. Taylor defended sociology without defining what sociology is. It is 'the study of the behavior of persons in relation to each other', but this hardly marks out a distinct domain. It is a collection of data, but it shares these with 'men of action'. It is a set of generalisations, but the more successful these are the less useful they become. It is a form of training, but this goes beyond inadequacy to be actively prejudicial. It is a set of methods, but these are the common stock of the social sciences. It is distinct from those disciplines, but is incomplete without them.

This contrast between sharp imaginary and fuzzy practice is a joint effect of the distinction of governmentality from government and of disciplinarity from discipline. Taylor was uniquely placed to exemplify both.

Fairchild (1937) had noted that sociologists were poorly represented in Roosevelt's administration, but Taylor was an exception who proved the rule. Shortly after Roosevelt's first election he moved from academia to Washington, rising from Assistant Administrator of the Resettlement Administration to chief of the Division of Farm Population and Rural Life within the Bureau of Agricultural Economics. Under his direction, the Division was increasingly well-funded and prominent practitioners were attracted to it. It became the largest employer of sociologists in the government

³ In one of the points which Kuhn did not take from him, Fleck (1935) had suggested just this circularity.

(Taylor 1939: 225), being involved in the identification of need, the administration of relief, and the planning required by the large-scale projects of the 1930s and 1940s (e.g. Taylor 1946b). He was well prepared for this. Rural sociology was more openly dependent on the state than most sociological specialities (cf. Anderson 1959; Hooks 1983), and the use in it of social surveys entailed just the targetting of populations and just the form of *savoir* of Foucault's 'governmentality'.⁴ He referred often to his twin loyalties during this period. Holding that sociologists should assume their "just role[s] as counsellor[s] to practical men", he saw the establishment of sociology in government service as an official duty (Taylor 1937: 483; 1942: 89). More pragmatically, while his Division had important work to do it was also a sheer source of jobs for sociologists, and "we should capitalize on the situation" (Taylor 1940: 460). His prominence in the discipline appears to have been at least partly an effect of his governmental position; through the New Deal and the wartime expansion of government, he was both a formal liaison figure between the ASS and the public service and an informal source of advice on how sociologists might join it (e.g. Folsom 1941; Taylor 1940, 1942, 1945).

The relation between discipline and government was particularly salient during the year of his presidency. Following wartime success in research, the proposal to create the National Science Foundation was before Congress, and the inclusion in it of a Division of Social Science was heavily contested; natural scientists decried the claims for social analysis, while members of Congress were wary of a stress on such politically awkward questions as racial inequality (Miller 1982). Taylor (e.g. 1946c) led the ASS' campaign for inclusion. So if his positions in the discipline and the government were related, then he achieved the peak of disciplinary eminence at the precise time that his own bureaucratic position was most under threat and that the discipline was about to endure a political and bureaucratic rebuff. For throughout the 1940s his Division had come increasingly under attack, in Congress and from rural lobby groups,⁵ and in the early 1950s both it and its parent Bureau were dismantled (Capener 1975: 400). The

⁴ Picou *et al.* (1978) and Harper (1991) described the use of surveys as a form of 'methodological monism' in rural sociology. In an extract from his doctoral thesis, Taylor (1920) had published an early defence of them.

⁵ Exception had been taken to studies conducted under his aegis, particularly to the finding in one that large-scale agriculture in California was detrimental to political and economic democracy, and to the support in another for black sharecroppers in Mississippi (Hooks 1983: 395-6).

campaign by and for social scientists proved similarly unsuccessful.⁶

Taylor then exemplified both poles in both elements of the analogy between governmentality/government and disciplinarity/discipline. Rohrer's (1970: 26) description of his address as "optimistic, activist, pragmatic, and oriented toward the future" could be applied as much to his governmentality as to his disciplinarity. But in the defeat of his administrative programmes, in his own later sociological obscurity, and in the exclusion of social inquiry from the National Science Foundation, he enacted the joint failures of government and discipline. In that sense, he is characteristic in his singularity. While no other president matched him in living both governmentality and disciplinarity, and so having what Mannheim (1932: 106) called "a maximum opportunity to test and employ the socially available vistas and to experience their inconsistencies", all were in some form of governmental service: as teachers at State universities, as recipients of grants, or as advisers, experts and policy-makers. All were subject to the same dual tension between optimism and failure.

All, too, were concerned with sociological knowledge. It would be too strong to generalise from the experience of inconsistency to overt attention to knowledge, but Taylor's standing as both the most governmental president and the leading user of the word does point to the layered effects of disciplinarity. Again he is typical in that the sociology of knowledge emerging from tensions within and between its three moments remained tacit, for despite his assumption of and attention to the existential determination of sociological knowledge he did not refer to the (sub)discipline. On a slight shift of focus, he then exemplified his own argument on common sense by being a paradigmatic folk sociologist of disciplinary knowledge.

It was concluded in the previous chapter that although the presidents were situationally debarred from the reflexivity required in a sociology of knowledge they invoked instead that Baconian 'folk disciplinarity' which Taylor epitomises. It will be shown in the following chapter that they also have a means of 'folk disciplining'.

⁶ The Division of Social Science was excluded from the original National Science Foundation in 1946. When the Foundation's Organic Act was passed four years later it included a provision that social research was permitted but not mandatory. It was not until 1958 that an Office of Social Sciences was established, and another ten years passed before the Organic Act was amended to bring the social sciences fully under the Foundation's legislated responsibilities (Miller 1982).

CHAPTER 8 : FOLK DISCIPLINING

The argument that presidential addresses are a folk sociology of sociological knowledge derives from the definition of a sociology of knowledge as a fractal ordering of disciplinarity, disciplining, and discipline. The folk disciplinarity of presidential addresses having been shown in the previous chapter, this chapter is devoted to folk disciplining. Disciplining was defined as the realisation of discipline from optimistic disciplinarity and the renewal of disciplinarity in the face of failing discipline. That circularity occurs in presidential addresses, and in a way which resolves the puzzle set by the enactment in the addresses of 'Mannheim's paradox': that the presidents are situationally debarred from what they are situationally required to fulfil. It will be shown by the rhetorical devices of genre, the topics; and metaphor/intertextuality that the presidents have a practical solution to the problem. While each device gives rise to a distinct form of disciplining, the first two are situationally constrained. It will then be argued that the 'folk disciplining' of the addresses is a tropical effect.

A presidential address generically implies a forensic policing of the discipline, but while the the presidents' 'normative repertoire' was used to index a disciplinary morality, the repertoire was not significant in the correspondence-analysis. This is consistent with the presidents' rhetorical constraints; the interpenetration of forensic, epideictic and deliberative requirements limits the directness of disciplinary policing. So 'generic disciplining' is expected to be discernible, but muted.

A supplement to it, 'topical disciplining', is also constrained. Denoting appeals to disciplinarity, or normative invocations of the topics of collectivity, usefulness and science, this will be illustrated in the presidents' attention to 'training', and in their use of the morally weighted charges of 'responsibility' and 'neglect'. Disciplinarity is invoked in each of these, but, again, mutedly so. Since what is meant by collectivity, science, and usefulness is situationally required to be left largely tacit, the presidents could make only restricted use of them.

That leaves 'tropical disciplining', or metaphorical and intertextual language, as the presidents' most viable form of policing the discipline. Being more diffuse in expression than either generic or topical disciplining, it is also more uncertain in its effectivity. It will be illustrated here through one image which recurs in the addresses - that of 'task' - with the presidents' use of it taken to epitomise a more general process.

After its salience has been suggested, three of its prime connotations - 'boundedness', 'religion', and 'work' - will be elaborated separately before specific usage is examined; used of, in, as and beyond sociology, 'task' is jaggedly trans-scalar, or fractal. Insofar as it exemplifies a general process, the presidents' disciplining of sociology will be seen as a mapping from other forms and sources of social order to that of the discipline. 'Folk disciplining' then entails the same epistemically flat processes as in the formal sociologies of knowledge.

GENERIC DISCIPLINING

Any occasioned speech implies the forensic moment of the classical typology of genres, and any genre implies a normative order. By their mere generic distinctiveness, presidential addresses imply that policing of the discipline defined as 'disciplining'. But since the forensic is always entangled with the epideictic and deliberative, generic distinctiveness also entails constraints on this generic disciplining. Already shown in the note on Coser's {1975} address, that effect will be elaborated briefly here.

While it was suggested in the case-study that no other negative sanctioning in the addresses was so direct and so personal as was Coser's, two other presidents, Queen {1941} and Lee {1976}, did match his acerbity. Their common normativity is already indicated in their titles, for they were the only presidents to use the bullying form of the rhetorical question. Having asked 'Can sociologists face reality?', Queen {1941} offered these examples of 'sociological flight from reality':

If a sociologist has difficulty in thinking abstractly, he may secure a grant to finance the counting of prostitutes, poolrooms and privies. If he dislikes meeting people, he may hide away in a library to assemble as many definitions as possible of human nature, society or psychosocial environment. If he finds research distasteful, he may devote himself to the students, appearing regularly at fraternity dances and football rallies. If the undergraduates annoy him, he may seek refuge in the reading of some European or American obscurantist...

Looking back to Lynd's (1939) 'Knowledge for what?', Lee {1976} asked: 'Sociology for whom?', and his own answer of 'sociology for humanity' involved sustained critique of the discipline. His 'basic issue' was:

whether or not so many sociologists must follow the same hypocritical career pattern as so many clergymen, lawyers, merchants, educators, labor union executives and manufacturers. Must sociologists use the moral or ethical pretensions of sociology as a cloak or a mask for their role in a social conspiracy? Must sociologists share in the control and manipulation of the masses through appearing to practice a humanity-serving discipline and through actually serving - whether they know it or not - narrow interests of society's ruling elites?

In other words, to what extent are sociologists part of a culturally enslaved instrument of exploitative control and of technocratic manipulation ?

But Queen's and Lee's harangues were as atypical as Coser's, and for the same reasons. The sense of inappropriateness evoked in reading them now must have been even more stark on their delivery. An after-dinner audience does not expect to be hectored. Since successful rhetoric requires the engagement of an audience, and since over-enthusiastic policing distances rather than engages, the addresses' occasioning calls more for soft soap than for Savonarola. Over-splenetic disciplining is almost certainly ineffective disciplining. That is the limit on its generic enactment.

To be sure, most addresses do include 'disciplining' passages. From Chapin's {1935} claim that reviewers of a particular case-study 'display[ed] an astonishing ignorance of scientific procedures [amounting to] indecent intellectual exposure', through Homans' {1964} call for sociologists 'to put an end to our intellectual hypocrisy', to Peter Rossi's {1980} contention that 'low quality and irrelevance are characteristic of most of the work of our discipline', the presidents routinely patrolled the practice of sociology. But they rarely showed the likelihood of losing their audience.

Their disciplining was more indirect. It is evident from the passages above that disciplinary boundary-riding also involves appeals to the relatively natural worldview ordered fractally under the disciplinarity of sociology. That is the process denoted by 'topical disciplining'.

TOPICAL DISCIPLINING

The topics of disciplinarity are by definition commonplaces of the discipline. Collectivity, science and usefulness, separately and as an ensemble, comprise a resource for the presidents' normalising duty. This 'topical disciplining' will be shown in two ways: in the presidents' concern with the training of sociologists, or 'disciplinary disciplining'; and in the 'disciplinary morality' implied by their use of 'responsibility' and 'neglect'. Each of these will be seen to entail the three moments of disciplinarity. But here, too, the presidents faced the constraints arising from the agonistic character of the topics and from the rhetorical requirement for consensus.

Disciplinary disciplining

The formalisation of training is one mark of a successful discipline. That was Ellsworth

Faris' {1937} point when he contrasted the 'irregular' emergence of early sociologists with the graduate training of his day, and the issue recurred throughout the addresses.

No other president was as mordant as Goffman {1982}:

We can't get graduate students who score as high as those who go into Psychology, and at its best the training the latter gets seems more professional and more thorough than what we provide. So we haven't managed to produce in our students the high level of trained incompetence that psychologists have achieved in theirs, although, God knows, we're working on it.

But around one third of them addressed or alluded to the issue, and their comments refer clearly to the disciplinary topics: sociology is scientific, useful, and collective.

The first president in this study, Chapin {1935}, put the lack of 'dependable instruments of social measurement' down to 'the absence of basic scientific training of young students in the social sciences'. For Stouffer {1953}, the slow development of such instruments, and thus of the experimental method enabled by them, was due to 'the lack of accumulated and transmissible experience in practical arts of handling people in the design and execution of an experiment'. But the growth he expected from more trainees themselves becoming trainers might have been too successful. Thus Coser {1975} held:

that we tend to produce young sociologists with superior research skills but with a trained incapacity to think in theoretically innovative ways. Much of our present way of training ... encourages our students to eschew the risks of theoretical work and to search instead for the security that comes with proceeding along a well traveled course, chartered (*sic*) though it may be by ever more refined instruments of navigation.

Sophisticated methods within the discipline were of little use if findings generated through them could not be assimilated. To enable that, Lieberman {1991} called for 'an appropriate epistemology' for the discipline. This was:

central for training our students because [while] they are trained in statistics and methods of research ... they rarely consider such epistemological questions as: How do we know what we know? When is our evidence reasonably conclusive?...

But however the scientificity of sociology was understood, a problem posed by Lundberg {1943} remained: 'how to trademark a true social scientist'. Such identification was necessary if sociologists were to be truly useful.

Presidents treating the 'usefulness' of sociology were as concerned with its teaching as those who focused on 'science'. So for Donald Young {1955}:

The way sociology is taught ... is poor preparation for work in any specific applied field. A society in which any man may be his own sociologist without external criticism or inner doubts because of lack of relevant training is an unhappy setting for a career in applied sociology.

But inclusion of applied sociology in graduate training might lead to inner doubts and

external criticism becoming disciplinary controversy, as Lazarsfeld {1962} noted of his 'research institutes'. The issue remained unsettled. Twenty and thirty years after his discussion of it two of his former students reiterated his call for the institutionalisation of applied social research. Peter Rossi {1980} presented applied work as a source of jobs when the academic market was shrinking:

In order to take greater advantage of these occupational opportunities for our undergraduate majors and those who earn graduate degrees, it is necessary for the discipline to build linkages to applied social research as an institutionalized activity. ... We will also have to revamp our undergraduate and graduate curricula to reflect a greater emphasis on quantitative research and on policy-oriented research ...

Making the point less pragmatically, Coleman {1992} still used similar language in his call for the 'rational reconstruction' of both society and sociology. That would involve:

training sociologists, both undergraduates and graduates, to be the architects and architectural aides in the design of social institutions. It implies an overhaul of the curriculum in sociology, with a new core focused on institutional design and the attendant policy research it requires.

Training in sociology was then to be both scientific and useful. But its place in sociology as collective was also at issue.

The trademarking of the 'true social scientist' for which Lundberg had called, and thus the reshaping of 'collectivity', was an element in the professionalisation of the ASS in the late 1950s and early 1960s. The move was not universally welcomed. Hughes {1963}, for example, held that:

While professionalizing an activity may raise the competence of some who pursue it by standardizing methods and giving license only to those who meet the standard, it may also limit creative activity, by denying license to some who let their imagination and their observations run far afield, and by putting candidates for the license (Ph.D.) so long in a straitjacket that they never move freely again.

Lee {1976} quoted that when he attacked constraints on sociological knowledge. Both disciplinary training and disciplinary careers, he held, hindered critical imagination:

Few enough of the creative and disputatious who manage to transcend graduate indoctrination, even exploitation, survive the years of non-tenured courtiership that precede the magical 'continuing contract' of the tenured and, eventually, the full professorship.

Gans {1988} likewise mentioned 'the ways we still often mistreat graduate students and part-time instructors which is in part a reflection of long-standing inequalities within the discipline'. The collectivity of sociologists, that is, was not structurally different from its environment

So the presidents commonly argued that training in sociology, disciplinary disciplining, should lead to work which is scientific, useful, and collective. But that

common ground remains so only as long as those moments are left hazy. A similar sense pervades the 'disciplinary morality' in the addresses.

Disciplinary morality

The addresses' disciplining moment was indexed by the 'normative repertoire' in the content-analysis, where it was found that the presidents used 'must', 'ought' and 'should' to a relatively significant extent. That moment will be illustrated here in two themes, 'responsibility' and 'neglect'. Recurring throughout the addresses, these will be taken as epitomising a 'disciplinary morality'. Their normativity is evident in this passage from Angell's {1951} call for greater attention to international sociology:

Almost all the important questions are unanswered. And they are not questions merely of theoretical interest to a handful of academicians. They are questions that concern the destiny of common people around the globe. It is deplorable that relatively few sociologists have appreciated this enormous challenge and that even fewer have done anything about it. It is my conviction that we must repair this negligence. We must awaken to our responsibilities. In these fateful times there are few groups that have so much to offer the world.

Angell's assumption that sociological knowledge is a collectively warranted fusion of theory and practice - that is, that disciplinarity is a moral question - is paradigmatic.

Recalling the origins of the ASS, Ellsworth Faris {1937} referred to its founders as 'largely concerned with the question of the knowledge, information, and point of view that was of value to American students and which it was the duty of sociology to contribute'. That sense of obligation recurred. It might be assumed, as in Parsons' {1949} claim that '[b]oth the urgencies of the times and the nature of our American ethos make it unthinkable that social scientists as a professional group should shirk their social responsibilities'. Duty might be given in the discipline's subject matter, as in Robert Faris' {1961} call for a sociology of talent to meet the needs of social coordination: '[r]esponsibility for research in this superorganic form of creative potential must of course be accepted by the science of sociology'. Or, as Wilson {1990} suggested, it might follow from the discipline's scientificity:

With the reemergence of poverty on the nation's public agenda, researchers have to recognize that they have the political and social responsibility as social scientists to ensure that their findings and theories are interpreted accurately by those in the public who use their ideas. They also have the intellectual responsibility to do more than simply react to trends or currents of public thinking. They have to provide intellectual leadership with arguments based on systematic research and theoretical analyses that confront ideologically driven and short-sighted public views.

Or, holding that social problems required sociological solutions, and that the authority

of sociology then needed to be both soundly based and recognised as such, Lundberg (1943) argued that sociologists had a responsibility to develop their scientificity:

We need to recognize that it is not authority as such that we need fear but incompetent and unwisely constituted authority. When we undertake to insist on the same criteria of authority in the social as in the physical sciences, no one will worry about the delegation of that authority any more than he worries about the physician's authority.

Peter Rossi (1980) stressed the consequences of sociological knowledge being taken as authoritative:

Applied social research often demands greater technical skills than does basic research. Because [it] may be used in the political process, it is clearly important that it be done well. After all, an article in a major professional journal, or a monograph, has little consequence except upon the career of the writer and, perhaps, except for attracting the attention of the handful of other social scientists who have been doing work on the same topic. In contrast, the product of applied social research might be used in the formation and change of public policy; and an error in applied work might have consequences not only for the social scientists involved but also for institutions, agencies, policy makers, and the intended beneficiaries of the policies in question.

So the 'responsibility' of sociology is directed both outwardly and inwardly, to science and to usefulness, and with the two strains reconciled in the collectivity enacting them. The same sense appears in the presidents' use of 'neglect'.

The weight of a charge of 'neglect' is evident in the presidents' responses to it. Denying that conflict theory was a radically new alternative to functionalism, for example, Loomis (1967) held that 'the study of conflict has been with us for a long time and no major theorist has neglected it'. Conversely, Komarovsky (1973) granted the force of criticisms of 'role analysis', that it 'is said to obscure and neglect the importance of individuality' and that within it 'deviation, malintegration and social change have been minimized or neglected'. The same normativity is evident in, say, Donald Young's (1955) acknowledgement of the 'danger' that applied research might 'lead to neglect of fundamental questions', in Moore's (1966) suggestion that 'fear of fallacy (the group mind) has led to the neglect of such realities as national purpose, including individual sacrifices for its realization', or in Alice Rossi's (1983) charge that '[t]heories that neglect [sexual dimorphism] carry a high risk of eventual irrelevance'. But if these passages are invocations of a morally weighted taken-for-grantedness over sociology as a useful science, they are so to a muted degree; few presidents associated 'neglect' with such strong language as the 'deplorable' in the passage from Angell (1951) with which this section opened. That is the overall effect of 'topical disciplining'.

Topical disciplining: Reprise

In the 'disciplinary disciplining' of their remarks on training and in the 'disciplinary morality' of their normative language, the presidents do use the topics of disciplinarity. But these topics are separately agonistic and interactively conflictual: reconciliation of science and usefulness would require re-fusion of the epistemological and political senses of representation; the presidents' imaginary of scientific sociology does not sit easily with sociology as a moral community; resolution of usefulness and collectivity would entail an unlikely political accord. Achievement of a more or less coherent ensemble from the topics is then difficult enough, and when the presidents' individual solutions are filtered through their obligation to appeal to disciplinary common sense the difficulties are compounded. Relying always on their audiences, the presidents are constrained from campaigning too strongly for the imaginary of sociology recognised in their election. But a solution to that difficulty remains. The normativity given in the genre and repeated mutedly in appeals to the topics of disciplinarity is also expressed more indirectly, through 'tropical disciplining'.

TROPICAL DISCIPLINING - TASK

In Chapter 7, Williams' [1958] quoting of Ward's founding presidential address to the ASS was used to show the permanence of 'science' as a disciplinary imaginary.

Ellsworth Faris [1937] also referred to that first meeting of the Society. He recalled:

the protest against those who were continually raising for debate the question whether sociology was a science. They considered it a profitless inquiry then, and there are those of us who consider it a profitless question today. It can only lead to barren and formal definitions and neither adds light to our path nor gives us assistance in our tasks. Complete, unified, consistent, sociology is not, as all the world knows. But these thirty years have seen the clearing away of much ancient error, the invention and perfection of many promising methods, the successful attack on many significant problems, and a gratifying growth in the number of men and women who are devoting their efforts to the creation of a sound sociological body of organized principles and tested laws.

In this one passage, Faris - and it might be recalled that he appeared among the most 'disciplinary' presidents in the correspondence-analysis - exemplified the presidents' situated denial of reflexivity and a means of fulfilling their situated requirement for disciplining. His defences of the readily apparent collectivity, science, and usefulness are developed by imagery rather than by directly normative appeals to disciplinarity, and the ensemble has a coherence beyond his picturesque mixing of metaphors. This

metaphorical and intertextual enactment of what is generically required but topically foreshortened is the process generalised as 'tropical disciplining'.

Fractal ordering was in part derived from the jagged systematicity of metaphor and intertextuality. Writers achieve effects by evoking the already read, and readers achieve coherence through an embodied response to the evoked, and if the relation between written and read is always contingent it is stable to the extent that writers and readers share a relatively natural worldview. The presidents' disciplining of sociology in this sense is then a matter of their linking disciplinary order to whatever other forms or sources of order their audience can take for granted. Since these are as legion and as interwoven as are metaphors themselves, the process could be followed from any number of starting-points. That chosen here, Faris' 'task', was selected textually-ethnographically, first by observation of its recurrence, and then through bafflement at its meaning. That non-arbitrarily contingent selection was made on the assumption that similar results would emerge from other choices; each of Faris' images of 'profitless', 'path', 'barren', or 'light', for example, suggests a rich network of associations. So the representation of 'task' which follows is representative, not definitive.

Task

If the disciplinarity of sociology comprises relatively natural assumptions over the scientific, useful and collective study of social life, and if the common sense of the discipline situationally requires evocation rather than specification in the addresses, then 'fairly coherent' disciplinary metaphors should be expected. The image of 'task' meets that criterion. Almost three-quarters of the presidents used it, and both in itself and as the focal point of clusters of metaphorical and intertextual connotations its usage implies the normativity ascribed to 'disciplining'. Before its associations are shown, its referents in disciplinarity are sketched.

Znaniecki {1954} suggested the morality and trans-scalar effectivity of ordered 'collectivity', 'science', and 'usefulness' when he held that most sociologists:

ignore the enormous multiplicity and complexity of social phenomena developing on the national scale, the continental scale, and the world-wide scale, as well as the historical background of these phenomena. They seem to be unaware that the most urgent task of sociologists is to investigate these phenomena and find some solution for the pressing practical problems which they involve.

Presidents who emphasised 'science' rather than Znaniecki's overwhelming 'usefulness' still used similar language. In defending 'reasoned inquiry', for example, Bendix {1970}

looked to a Weberian sense of sociology as a vocation:

Like the great rationalists before him, but with none of their optimism, Weber commits himself to the scientist's calling. For him science is the affair of an intellectual aristocracy. It demands concentration, hard work, inspiration and the passionate devotion to a task that can only be accomplished if all extraneous considerations are excluded.

Various suggesting urgency, centrality, commitment and neglect, these not atypical extracts illustrate a normatively infused conjunction of social order with the science, usefulness, and collectivity of disciplinarity. Given that set of associations and its recurrence in the addresses, 'task' appears to be a disciplining term. That is roughly confirmed by the fact that the president who emerged in the correspondence-analysis as the most 'disciplinary', MacIver [1940], also used 'task' most heavily. But if its disciplinary use is clear, its meanings are less so.

Textual-ethnographic observation of the term induced a search for its use in social scientific texts of the last century. It has twice been given a subdisciplinary meaning, in Taylor's (1911) 'scientific management' and in the 'technological paradigm' in organisational sociology (e.g. Dill 1958; Burns and Stalker 1961; Woodward 1965; Perrow 1967; Thompson 1967): a task is a duty within a formal division of labour.¹ But whether used in that specialised sense or more generally, its meanings cluster around the dictionary definition of arduousness and imposition. Since an imposed duty may be either transcendent or mundane,² three broad meanings of 'task' are separable: a morally infused sense of 'duty in a calling'; work in itself; and a sense of the bounded collectivity involved in either form of division of labour. Each of these is recurrently identifiable in the addresses. As MacIver [1940, emphasis added] said of 'sociology in a time of crisis', '[w]e need sustained devotion and we need intellectual sweat'.

Those connotations of 'task' will be elaborated in the three subsections below. The tropical disciplining given in the term entails the drawing of links between the order of sociology and images of 'boundedness', of 'religion', and of 'work'.

¹ Taylor (1911: 39) put 'task' at the core of his method: "[p]erhaps the most prominent single element in modern scientific management is the task idea". Burns and Stalker's (1961: 120) 'mechanistic' form of organisation was characterised by a "specialised differentiation of functional tasks into which the problems and tasks facing the concern as a whole are broken down". They had a precursor in Ure (e.g. 1835: 17-18, 20-21), whose imagining of the 'regulated task' of workers within the fully automated factory were to evoke some of Marx's more savage footnotes.

² The mundane is evident in any of the accounts of the division of labour. *The Protestant Ethic* is paradigmatic of the transcendent, for whether the language was Weber's or Parsons', 'task' recurs in the version of it familiar to generations of American readers. In his genealogy of *beruf*, Weber (e.g. 1904-5: 79, 85, 88) repeatedly talked of work in a calling as a task, or *the* task, set by God.

'We' - Boundedness

Use of the first person is a generic feature of presidential addresses, and that was combined with images of 'community' in Chapter 7 to stress the collective moment of disciplinarity. But while 'the discipline' has been shown as a moral community, it has also been found that as the presidents widely cited writers from other disciplines, the boundaries of sociology were unclear. Since disciplinary specificity is assumed in such asides as 'the task of sociology is to discover regularity in social life' (Williams 1958), or that '[o]ur sociological task is to account for aggregate phenomena on as general a level as we can' (Komarovsky 1973), the language used to depict relations between sociology and other disciplines is a means of policing collectivity. Whether sociology is 'the generalizing science of man's social behavior' (Moore 1966) or 'the residual social science' (Gans 1988), or whether it had 'a focal role to play' in global politics (Angell 1951) or warranted Ellsworth Faris' (1937) account of 'the degradation of sociology, to a position of secondary importance', interdisciplinary language is disciplining language. Goode's (1972) apologia for his attention to 'force' is paradigmatic: if that 'is properly the territory of our sister discipline, political science, they (*sic*) have not bothered to cultivate it; and we shall not be usurpers'. In this one normative aside ('properly') are the 'work' ('cultivate') to be treated below, and images of 'blood' ('sister'), of the 'geopolitical' ('territory'), and of 'ownership' ('usurpers') which all recur throughout the addresses.

By 'blood' is meant familial relationships, and more specifically, gendered relationships. While both male and neutral descriptors were used, as in Queen's (1941) 'sociological fraternity' and Cottrell's (1950) 'social psychologists and their close kin [sociologists]', disciplines were more usually depicted as female. Parsons (1949) spoke of 'sister sciences' and Cottrell (1950) of 'our esteemed sister society [the American Psychological Association]'. Ellsworth Faris (1937) was most explicit, alluding to 'sister disciplines', and, with reference to its differentiation, saying that '[i]f in the past sociology was daughter in her mother's house, today she is mistress in her own'.³

As in Cottrell's 'esteemed', the connotations of 'sister' and of femaleness in

³ MacIver's use of 'mistress' in another sense - "Sociology has always been for me a kind of beloved mistress with whom I seemed unable to get on really comfortable terms" (quoted in Bramson 1970: 12) - is consonant with a peculiar effect of this femaleness of 'discipline'. As might be expected, the generic male was used all but universally until the latter part of the period under study. Since men as a collectivity are then female - Fairchild (1936) provided a rare exception, when he spoke of 'a sort of unwritten Gentleman's Agreement' between economics and sociology - 'discipline' appears in its sense of leather and the lash.

general are largely cordial, and that same cosiness is found in spatial images. Thus Queen {1941} talked of 'our neighbors, the psychiatrists', Williams {1958} of sociology's 'neighbors among the social sciences' and Riley {1986} of 'neighboring disciplines'. In their contexts these are invocations of suburban *Gemeinschaft*, with each discipline, a family in itself, enjoying friendly relations with the others. But neighbours can be more than that, as in Fairchild's {1936} depiction of economics and sociology:

Are [they] two distinct sciences ... like two neighbors who have a polite bowing acquaintance and pass the time of day over a sturdy boundary fence, but each of whom would vigorously resent any trespass of the other upon his territory ?

The spatial metaphors here slide from the familial to the geopolitical/military. The connotations of 'boundary', 'trespass' and 'territory' in the latter sense reappear in a number of usages, and with them a range of further implications. The frequently used 'field', for example - as in Sanderson's {1942} reference to 'the border fields' between disciplines or in Parsons' {1949} allusion to 'our own field' - may be either agricultural or geopolitical. Or it might be explicitly both, as when Ellsworth Faris {1937} held that 'some of the richest fields for cultivation lie on the frontier between two specialities'.⁴ Likewise, when Goffman {1982} spoke of '[s]ocial anthropology claim[ing] ... ceremonies as its province', the image suggests both an an imperial centre and the allocated duties of a division of labour. The former is also made explicit, as in Parsons' {1949} noting of 'the unfortunate clash of sociological and anthropological imperialisms', or in Riley's {1986} claim that '[w]hile recognizing that aging is in certain respects the proper subject matter of both biology and psychology, [the sociology of aging] denies frequent imperialist claims of these disciplines'. Gans {1988} deprecated the image even while asserting it: '[d]isciplinary boundaries in the social sciences are arbitrary anyway, and they should be crossed freely, preferably for substantive, not imperialistic, reasons'. Empires, of course, are held by force, but even an empire might not be able to fight its battles alone. Fairchild {1936} talked of 'the allied social sciences', and Riley {1986} of 'alliances of sociology with economics, political science and anthropology'. Less belligerent relations are also possible; Ellsworth Faris {1937} saw interdisciplinarity as 'a democracy - a cooperative quest, where each can get from others and give back, in return, something of value'.

This hint at an invisible interdisciplinary hand recurred. As the universally used 'our(s)' suggests, a discipline is a possession, but it is more; it is a business. Parsons

⁴ Given the place of 'frontier' in American history, this has a further tinge of progressivist expansion.

{1949}, for example, referred to a 'general stock-taking of the state of our discipline', Donald Young (1955) called for more involvement with the 'practicing professions', as '[i]t would be a pity ... not to take advantage of the opportunity to test our 'wares'', and Peter Rossi (1980) claimed in his defence of applied sociology that:

there has been considerable commerce between the two parts of our discipline, to the considerable enrichment of both. ... the commerce between basic and applied work is certainly a two-way exchange. No-one knows whether ... the balance of trade favors the one or the other side of the exchange.

As an economic unit becomes a political unit, the economic metaphor merges with the geopolitical, for possession implies a potential struggle. Lee (1976), for example, held that 'the credibility, privileges and opportunities of sociological work constitute a kind of territory over which professional practitioners and, to a lesser extent, politico-economic interest groups contend for influence and control'.

So the relatively natural disciplinary worldview entails more than the science and usefulness of disciplinarity. Or rather, since the scientific and useful 'tasks' of sociology, those which distinguish it from other disciplines, are policed through all the connotations and intertextuality of 'blood', 'territory' and 'ownership', they are always already infused with collectivities beyond the strictly disciplinary. The bounded 'we' is potentially unbounded. Metaphors of the sacred yield the same effect.

Sustained Devotion - Religion

The transcendent moment of 'task' - MacIver's 'sustained devotion' - is evident in the religious imagery pervading the addresses. Since Comte's 'positive knowledge' replaced the 'theological', sociology was in one sense defined against religion, yet American sociology was also derived in part from the progressivist social gospel. The consequent tension is clear in Sanderson's (1942) defence of sociology as a 'means of democracy'. After describing democracy as 'a moral issue and essentially a religious faith', he added:

If sociology is to make fundamental contributions to the urgent demand that science be applied to the complex problems of human relations which now face us in realizing our faith in democracy, then it must use the established methods of science ... The phenomena of sociology are the forms of human association. Its task is to describe those forms of human association that are sufficiently recurrent under given conditions to make the behavior of human collectivities predictable.

Other presidents also implied that sociology as a useful science was a religiously justified replacement for religion. When Lundberg (1943) depicted 'a legalistic and

moralistic viewpoint anchored in theology' as a surviving prescientific orientation in social life which sociology should replace, or when Hauser {1968} spoke of 'organized religion as a living museum of cultural atavisms adding to the confusion and disorder of contemporary life', religion was a Comtean Other for sociology. Yet it still served as a disciplinary template. Lundberg, again, deprecated the way that books by Marx, Weber, Durkheim or Pareto were treated as 'sacred texts'. In defending 'the utility of utopias', Moore {1966} claimed that 'Marxist theology has a hardy survival power, not because of its scientific accuracy, but because it is a worldly eschatology'; and Loomis {1967} too spoke of Marxism as one expression of 'a very great drive to build the Kingdom of God on earth'. Sorokin {1965} used 'denominations' and Coser {1975} and Lee {1976} used 'cult' and 'sect' to decry particular intradisciplinary formations. Lee also elaborated the image, in his treatment of training:

Novices quickly grasp the scenario, the ceremonials, what courtiers to the tenured need to 'understand'. In too many graduate schools, they come to sense that they are being initiated into a kind of secular religion replete with revered personages, revealed doctrines, rites of passage and ceremonials performed by the ordained.

But theological and ecclesial images were also used constructively. Bendix's {1970} Weberian connection of 'task' with the Protestant ethic and 'duty in a calling' was not unique; Wirth {1947} talked of the 'vocation' of sociologists, Sanderson {1942} and Queen {1941} of their 'calling', and MacIver {1940} and Moore {1966} of their 'mission'.

Biblical language is also common. From Ellsworth Faris' {1937} granting that '[c]riticism we have always with us, like the poor', through Becker's {1960} claim that 'while the lump of society had been leavened, the salt of sectarianism had lost some of its savor', to Erikson's {1985} contrast of Marx's understanding of work with the promise in Genesis that 'in the sweat of thy face shalt thou eat bread', scriptural imagery is used in accounts of the discipline and in substantive analyses. Implying a scripturally literate audience, and evoking any of the connotations those images have accumulated in the western tradition, such usages both imbue the occasion of the addresses with a solemnity beyond a gathering of professional colleagues and endow that collectivity with something of a pentecostal spirit. Perhaps the Bible is the prime source of intertextuality in any western writing, and perhaps many scriptural phrases have become so commonplace that their origins are forgotten or unknown. But even if their echoes have a dying fall, they are not dying, for the sacredness they imply is evoked by other means as well.

The presidential address, for example, was depicted as a sermon. Wirth {1947} and Gans {1988} used the word, while Taylor {1946} and Goffman {1982} presented their addresses as 'preachments'. A similar sense is evident in Homans' {1964} taking of the chance 'to speak ex cathedra' and again in his critique of functionalism: '[h]ere endeth the destructive part of the lesson'. Less explicitly but more evocatively, Queen's {1941} peroration had both the form and tone of revivalist exhortation:

let us neither hide away in an ivory tower nor sally forth with the idea that sociology will save the world. Let us pray that we may be spared both from inferiority feelings and from a Jehovah complex. Let us refrain from playing either with concepts or with unassorted facts. Let us guard against hasty generalizations, but having arrived at legitimate conclusions, let us offer them to the world for whatever they may be worth.

That Wirth, Homans, Goffman and Gans all adopted the image of the sermon with a degree of irony confirms rather than detracts from the sense of occasion suggested by Queen and Taylor. Further, if the audience/readership of the addresses is pictured as a congregation, the image of its location in a wider church is also implied. In contrasting the graduate training of sociologists with the 'parthenogenesis' of the founding generation, Ellsworth Faris {1937} cited the early Baptists having to baptise each other in order to comply with their own doctrines. Wirth's {1947} conclusion, that '[t]he circumstances under which we live do not any longer allow the saints to sit in their ivory towers while burly sinners rule the world', also casts sociologists among the elect, and Moore's {1966} 'plea ... for indulgence toward our brethren who think that man is worth saving and his lot in life worth improving' has a similar sense. Sociology is religious, too, in its virtue of 'humility' {Queen 1941, Stouffer 1953, Merton 1957}, and in its 'sins' {Coser 1975, Kohn 1987, Gans 1988}.

Sociology can be suggested as a priesthood. Ellsworth Faris {1937}, again, called for 'caution lest the layman overestimate our achievements and we be tempted to make premature pronouncements'. Moore {1966} justified his attention to the telic aspects of social life in similar terms:

Man has always been a problem-solving animal, but in the course of his social evolution he has increasingly invented the problems to be solved and the novel goals to be achieved, rather than merely coping with ambiguity and adversity. It is this addiction to discontent and to the search for a better future that I want to explore here, particularly as this activity of the laity relates to the enterprise of sociology as the generalizing science of man's social behavior.

Gans {1988} likewise devoted a considerable part of his address to 'our relations with America's nonsociologists, the lay public'. However secularised the term has become, traces of its ecclesial sense remain.

Perhaps most strikingly, sociology is the 'faith' suggested by Gans' (1988) inclusion of the discipline in his claim that '[i]dealized natural science is a kind of civil religion in modern America'. Sanderson's (1942) reconciliation of sociology with faith in democracy might be recalled, and even Lundberg (1943) held that once 'we':

put our undivided faith in science, we shall enjoy not only the support of a faith more demonstrably deserving our allegiance than many that we have followed in the past, but we shall also vastly accentuate the transition to the realization of that faith.

No president is apparently more distant from Lundberg than Goffman (1982), but he too appealed to sociologists to 'keep faith with the spirit of natural science'; Kohn (1987) likewise used his address 'to re-affirm [a] fundamental tenet of my scientific faith'.

Through these scriptural, ecclesial and religious metaphors the sociological 'task' is shown as missionary and the discipline then as a morally fused collectivity. Such tropes might be unavoidable. Like the scriptural intertextuality of western language generally, the language of social analysis remains suffused with traces of its theological origins. More specifically, the congregation still stands as paradigmatic of a ritual gathering and the sermon of an occasioned speech. Few other templates offer such a sharp conjunction of the celebration, ritual reaffirmation of the collectivity, and disciplining exhortation expected in presidential addresses. But if sociology is then tacitly located in the city of God, it is also to be found in the earthly city. The 'tasks' of sociology entail 'work'.

Intellectual Sweat - Work

Sociologists too earned their bread in the sweat of their brows; as Donald Young (1955) said, '[t]he community from which [they] draw their livelihood expects them to earn their keep'. Most of the presidents used the conventional 'work' to refer to their own and others writing, and images of 'work' recur throughout the addresses. These too are both disciplining and dispersive.

The images were drawn from any historical period since the hunter-gatherers of Merton's (1957) 'clan of historians'. In both its scientific and useful moments, sociology was agricultural. Ellsworth Faris (1937), for example, spoke of its 'richest fields for cultivation', Wirth (1947) held that 'the engineering of public consent is one of the great arts to be cultivated', or Bendix (1970) warned of excessive 'cultivation of judgment and sensibility in sociology'. Or, the metaphor of 'tool' is common, as when Chapin (1935) saw 'sound theory' as consisting of 'a logical system of relations among

concepts, postulates and hypotheses, all of which taken singly are so many provisional tools used to interpret experience in a meaningful manner', or when Lieberman {1991} spoke of 'information-gathering' as 'often a very sophisticated application of the existing theoretical and methodological tools of the discipline'. Reference to 'tools' might evoke the mediaeval image of the discipline given in Cottrell's {1950} appeal to 'members of our craft' or in Williams' {1958} contention that 'sociology requires not only a theoretic apparatus and a capacity to develop penetrating hypotheses but also a high order of research craftsmanship'. Relatedly, Merton {1957} spoke of 'us artisans of research', while Lee {1976} held as an ideal 'the individualistic artisan researcher' or 'artisan scientist'. Or the referent might be the industrial age, as when Lazarsfeld {1962} granted a criticism of his 'research institutes', that individuality may be lost in the 'research machinery'. Gans {1988} elaborated a similar image:

Although we are paid for the number of courses we teach, we are promoted by how much we publish, and only sometimes by the quality of our publications as well. In effect, our strange institution operates like a machine shop in which publications are treated like piecework like workers in other machine shops, we are periodically greeted by new technology, most recently of course the computer. ... Like many other industries, we too are becoming less labor-intensive.

Sorokin's {1965} appeal to 'machinery' drew on a different industry:

Thanks to the participation of a large army of researchers in collective research projects, 'the steam shovels' of numerous investigating crews have dug up an enormous mass of 'facts'. In this mass of facts now and then some grains of cognitive 'gold' have naturally been found; but the excavations have turned up only a few gold nuggets and have failed to strike a rich vein of new sociological knowledge.

But while sociology might be depicted as farming, as a craft or as one or another mechanised industry, one kind of work is most commonly suggested. When Stouffer {1953} said in his address on 'Measurement in sociology' that when faced with any research question '[w]e look into our carpenter's chest and find a good many tools', he articulated a common understanding: sociology is construction.

It is a recurrent theme that sociologists should construct sociological theory and sociological knowledge. Few presidents may have endorsed Taylor's {1946} view of 'common sense', but few would have disputed the tenor of his claim that sociologists 'will not, in fact cannot, perform their share of the common task of constructing a sound sociology out of a combination of science and common sense unless they continuously and fruitfully work in the field of social theory'. Parsons' {1949} aim, of linking 'theory' and 'research' by 'building the foundations for establishing more direct and specific connections', was a common concern. Any empirical work required a 'framework of larger

theory, even if that theory is a modestly light and shaky scaffolding capable of being blown down by the first gust of serendipity' (Stouffer 1953). Increasing specialisation made this more urgent, resulting in 'renewed efforts to build theoretical schemas intended to be capable of ordering the full range of sociological knowledge' (Williams 1958). This required 'an adequate conceptual framework' (Riley 1986). Or sociological knowledge might be imagined as 'basic building blocks', and, given pressing needs, '[i]t ought to be worth trying to determine what is the most fundamental knowledge needed to begin the building block process' (Lieberson 1991). Construction within sociological knowledge slid into the application of knowledge, as when Hauser (1968) held that sociologists should place the discipline among the 'as yet ... pathetically few professions' devoted to 'social engineering'.

As these images of construction show, and the same could be said of the images of work more generally, tropical disciplining does complement the topical; science and usefulness are both tasks to be achieved. But the same point has also been reached with 'intellectual sweat' as with 'we' and 'sustained devotion', in that disciplinary common sense cannot be restricted to the strictly disciplinary.

Tropical disciplining - task: Reprise

This section rested on three assumptions: that the recurrence of 'task' warranted it being read as a disciplining term; that it could then be taken as the focus of clusters of metaphors and intertextualities; and that these are performative. On those grounds, the addresses have been shown to be pervaded with images of a bounded 'we', of 'religion' and of 'work'. Each clustering of tropes supports the collectivity required of a discipline imagined as both scientific and useful, and to that extent this tropical disciplining reinforces the generic and the topical. But the familial, geopolitical and economic connotations of 'we', the ecclesial depiction and scriptural intertextuality of 'religion', and the cross-historical origins and layered applications of 'work' also all suggest modes of ordering which lie beyond the strictly scientific and which complicate the useful. 'Tropical disciplining' implies that the disciplinary is entangled with regularities which cannot easily be reconciled with it. Or, if the relatively natural worldview shown in this disciplining is more an effect of a general common sense of order than specific to the discipline, then sociology is seen as fractal. The trans-scalar character of the discipline's jagged self-similarity to its context is evident when the

direct referents rather than the connotations of 'task' are examined.

FRACTAL DISCIPLINING - TASK

In the exemplary passages early in this chapter, and in almost all the uses of 'task' quoted throughout, the word has been applied to the discipline as a whole; the task of sociology is this or that. But as Weber held, the idea of duty in one's calling prowls about in our lives like the ghost of dead religious beliefs, and so too it prowls about other referents of 'task'. The presidents apply the term across prepositionally separable scales. Apart from the broadly disciplinary referent noted above, 'task' is directed in the addresses to activities *within* sociology, it is used *as* sociology in one or another of the technical senses in the sociologies of work and organisation, and it is applied to the world apparently *beyond* sociology. On the continued assumption that 'task' cannot be used without its connotations being suggested, these extra usages are now illustrated, and their simultaneously recursive and dispersive effects as an ensemble taken as epitomising the disciplining moment in the addresses.

First, *within* sociology, 'task' implies an internal division of labour, as when Lazarsfeld {1962} held that in the organisation of research institutes '[t]he different roles must be made explicit; each has to know what is expected of him and how his task is related to the work of others'. It was applied to substantive neglect, as when Cottrell {1950} spoke of 'the obvious task of developing a reliable and sensitive index of the empathic ability', or to the need for theoretical development, as in Blumer's {1956} claim that '[a]lthough we already have appreciable accumulations of findings from variable studies, little has been done to convert the findings into generic relations. Such conversion is not an easy task'. It denoted occasioned limits, as in Hawley's {1978} claim that 'a comprehensive overview of the literature on social change would almost certainly bewilder the uninitiated person should that individual have the patience to undertake so arduous a task'. It was applied to disciplinary introspection, as in Gans' {1988} demand that 'we ought to begin on the... complex task of looking at what students *learn* from ... basic courses'. Or, finally, it referred to disciplinary organisation, as in a passage Queen {1941} quoted from a report by the Social Science Research Council:

Sharing deeply the concern of all Americans with public problems of great immediate

import, and watchful for opportunities of useful service in national emergency, the Council during the past year prosecuted steadily as its primary task its program for development of scientific knowledge of human society.

Sometimes the related 'division of labour' was applied explicitly to sociology. In his call for the matching of theory and research, Parsons (1949) held that '[t]his is a point where a division of labor is very much in order', and Hughes (1963) too claimed that he 'would like to imagine a state of things in which there would be a grand and flexible division of labor among us'. In Durkheim's centennial year, Williams (1958) most elaborated the recursivity of the image:

even the professional sociologist ... may wonder whether and to what extent there is unity in sociological studies. For the most part, however, this proliferation of particular topics represents an entirely normal division of labor and specialization of skills and knowledge. To what extent the division of labor represents the 'organic solidarity' we have a right to expect of a coherent field of study its itself a subject for sociological analysis. The present specialized interdependence will attain the impersonal unity Durkheim envisaged for society at large if, and only if, its disparate concrete concerns are approached by an accepted procedure (analogous to the 'rules' that lie outside of and antedate specific contracts), if its concepts are generic conventions, and if its goals express the common values of the search for invariant, intersubjective knowledge.

The 'common values' in these extracts variously suggest that an intradisciplinary division of labour is imbued with the same urgency and duty as found in allusions to the discipline as a whole. Tasks *in* sociology have the same sense of mission as the tasks *of* sociology. That connection is somewhat less explicit when 'task' is used technically.

'Task' was twice given a technical sense in the study of work: in Taylor's 'scientific management' and in the 'technological paradigm' developed from the 1950s. But while 'work' was an all but universally adopted source of imagery, few presidents referred explicitly to its subdisciplinary analysis. Blau (1974), as an organisational theorist, was among those who did:

Subdividing jobs narrows the range of different tasks of all jobs or many of them, but the total repertory of tasks remains presumably the same. For instance, if all clerks once both typed and filed, and now some only type and others only file, the range of tasks for each is narrower than before, but the tasks performed by the entire group have apparently not changed. Actually, however, the sheer subdivision of work often gives rise to new tasks.

Otherwise, usage in this sense is relatively sparse, and relatively late in appearance. When Williams (1958) spoke of 'unitary task accomplishment', Erikson (1985) of workers' 'increasingly reduced role when a task is broken up into minute segments', and Coleman (1992) of automotive workers 'making decisions about reorganizing the tasks on their line', their usage is technical, but the incidence of this meaning is far less than in

the other two. Still, that the technical sense is used at all adds 'task *as* sociology' to 'task *of* sociology' and 'task *in* sociology' as performative within the addresses. One final sense is noted, a residual category of 'task *beyond* sociology'.

To a greater extent than that of the technical, the presidents used 'task' with a range of broader referents. The word was treated as a synonym for 'activity', as in Queen's {1941} asking whether '[i]n avoiding the tasks for which we are admittedly not equipped, shall it be said that we escaping or facing reality?', or in Alice Rossi's reporting that 'the right hemisphere of the brain is dominant in emotions, facial recognition, music, visual tasks and identification of spatial relationships'. Many of these residual uses were variously political. Although Hanks {1938}, for example, warned that '[g]overnment is not an engineering job of getting a clearly visualized task done with the minimum of effort and expense and in harmony with the established scientific principles', Angell {1951} argued for a 'world society':

the growth of a world federation requires that problems of international trade, finance, and migration be handled in terms of world moral norms, that the United Nations become the accepted instrument for resolving power conflicts among nations, and that tolerance be institutionalised as a positive norm in international relations. The first of these tasks is well along.

Or, in talking of the 'chaotic society', Hauser {1968} claimed that:

The task is ... to welcome disorder, both in Durkheim's sense of helping to define the limits of order and a symptom of needed change, but to control the levels of disorder, while effecting change, so that it does not threaten the viability of society.

Yinger {1977} quoted a similar call for accommodation of biologically derived 'hedonic individualism and self-centredness': '[t]he continuing task ... is to arrive at a minimax solution or a stable compromise between the needs and requirements of the biological and the social systems'. Huber {1989} asked '[w]hy do men and women do certain tasks, and which ones yield the most prestige and power?', and Gamson {1994} held in his 'politics of exclusion' that 'challengers to indirect exclusion are faced with the daunting task of mobilizing resources to change the policies and practices of powerful, well-organized and well-entrenched adversaries'. The transcendent collective purpose in these variously political extracts evokes the same missionary urgency as was found in the tasks *of* sociology.

So 'task' is used throughout the addresses *of*, *in*, *as* and *beyond* sociology. Since

this prepositional layering implies a continuity between the discipline itself, what is studied within it, how study is conducted, and the social world at large, it epitomises the fractal ordering derived from the formal sociologies of knowledge and expected if the addresses are tacit forms of them. Defined generally by the same externality and constraint as are the social facts of the discipline's attention, used technically in reference to the division of labour which produces them, and imbued with missionary and political connotations, 'task' evinces a layered recursivity and evokes a layered legitimacy through traditions which are both disciplinary topics and disciplinary resources. That articulating of polysemic and intertextual disciplinary givens is one means of disciplining. It is one means by which the reflexivity required in formal sociologies of knowledge is enacted in the folk version of the addresses.

Its effectivity depends on ambiguity or polysemy. All but eight of the thirty presidents who used the word more than once did so in more than one of the four senses noted here, and while the direct referent in each case was clear, connotations are less controllable. That is what makes 'task' a disciplining term. Since the presidents are epideictically required to link their specific concerns to those of the membership at large, concerns which are always already imbricated in a myriad of ways with the world beyond the discipline, they must use a language and draw on a web of meanings which are polyvalently intra- and extradisciplinary. In this sense, their use of 'task' is paradigmatic of fractally tropical disciplining.

This is one peculiarity of presidential addresses. The policing of disciplinary boundaries in them - disciplining - enacts the instability of discipline. Already found in the formal sociologies of knowledge, this is also characteristic of this folk version.

FOLK DISCIPLINING : REPRISE

The 'disciplining' of the formal sociologies of knowledge was defined as the set of processes by which 'discipline' was realised from optimistic 'disciplinarity' and by which 'disciplinarity' was renewed in the face of failing 'discipline'. Seen in three phases - generic, topical, and tropical disciplining - it has been found in the addresses.

Like any genre, that of the presidential address encodes a normative order, and it had already been shown that it also entails an explicit requirement for policing of the discipline. But such 'generic disciplining' is moderated by that rhetorical need for

adherence of an audience which makes the addresses a lived version of 'Mannheim's paradox'. It was seen that few presidents elaborated the forensic moment of their addresses, that those who did so left an impression of inappropriateness, and that they then risked disrupting rather than achieving the collective renewal expected of them. 'Topical disciplining', comprising appeals to the collectivity, science, and usefulness of disciplinarity, was one supplement to the generic. It was seen in the 'disciplinary disciplining' of the presidents' attention to graduate training and in the 'disciplinary morality' of 'responsibility' and 'neglect'. But here too they faced rhetorical limits. Given the contestability of science, usefulness and collectivity, and given the need for their audiences' adherence, they could not advance too strongly any one version of the disciplinary topics. Since the required reflexivity was again blunted, another forensic means was needed. This was found in 'tropical disciplining', or the metaphorical and intertextual linking of disciplinary order to social order in general. The presidents' use of 'task', as both a focus of clusters of connotations and a multi-focal term in itself, was taken to epitomise the process.

The connotations of 'task' were derived from the president who most used it. The 'we' of MacIver's {1940} claim that as sociologists '[w]e need sustained devotion and we need intellectual sweat' led to 'boundedness', his 'sustained devotion' to 'religion', and his 'intellectual sweat' to 'work'. Taken as the centre of a cluster of related tropes, each image was found to entail an apparently aporetic effectivity. The more that disciplinary boundedness was asserted, the more the metaphors implied the opposite. The folk disciplining through scriptural, ecclesial and religious images gave religion as a justification for a disciplined replacement for religion. Images of work were likewise drawn as much from preindustrial times as from the modernity of sociology's location. The at once recursive and dispersive effect of the three central tropes was extended, when, on an assumption that 'task' could not be used without its connotations being implied, its referents of, in, as, and beyond sociology were examined. Through its trans-scalar application, tropical disciplining is fractal disciplining.

The relatively natural worldview given in the collective, scientific, and useful moments of sociological disciplinarity is then an effect of other forms and sources of social order. Since direct appeal to these would lead to open contradiction, the more indirect their invocation, the more they are amenable to disciplining use. That is a regularity in the apparently irregular mixing of metaphors found in all the addresses.

All or any of the forms of organisation, or all or any of the themes around which social analysis has been developed - religion and the Bible; work; the family; political, military and economic formations - are open to use in tropically disciplining appeals. The unity of their effectivity - order - derives from dispersive orderings. The tropes seen in this chapter are scattered enough, and they are by no means comprehensive. 'Task' was taken as illustrative rather than as foundational. Since traces of any of Barthes' (1971: 146) "innumerable centres of culture" can be found within the addresses, it was not selected for uniqueness of effect.

The fractal performativity found in 'task' could also be derived from each of the connotations noted in this chapter. Each of the images of 'religion', 'blood', 'work', 'economy', or the 'geopolitical' might itself be taken as a point of departure and its use of, in, as and beyond sociology elaborated. Other terms used frequently in the addresses - terms such as 'control', 'orientation', or 'perspective' - would lead to the same result; like 'task', each implies varied connotations, and, like 'task', each is used of, in, as and beyond sociology. Through using them, and any number of others, the presidents resolve the occasioned debarment of their occasioned requirement for policing of the discipline. Given the limits on generic and topical disciplining, they use whatever they have to hand to invoke disciplinary order, and what they have are all other forms and sources of social order. This is the epistemic flatness of sociological knowledge. As in the formal sociologies of knowledge, and with the same sense of 'construction', 'nothing special' happens in this folk disciplining.

The argument that as representatives of the discipline the presidents act as sociologists of knowledge has been advanced by the layered accounts of disciplining in this chapter. The jagged and trans-scalar performativity found in the formal sub-/metadisciplines has recurred in the folk disciplining - generic, topical, and tropical - by which the presidents fulfil what is required of them as representatives. After the implications of 'task' are further illustrated in MacIver's {1940} address, the argument is continued in the next chapter, on the 'failing' of 'folk discipline'.

CASE 8 : MACIVER'S TASK

MacIver's [1940] use of 'task' alone makes his 'reflections on sociology during a crisis' worth attention, and his address has other advantages. MacIver most forcefully invoked Bacon¹ - who was imagined in the tradition of the sociologies of knowledge, and whose imaginary of collectivity, science, and usefulness was used in 'disciplinarity' - and, appearing in all three dimensions of the correspondence-analysis, he was the most 'disciplinary' president. The conjunction of those features allows access to the generic distinctiveness of 'task, to the disciplining effectivity of its connotations, to the relation of Baconian disciplinarity to the sociology of knowledge, and finally, to the contingent interrelation of disciplinarity, disciplining and discipline: MacIver may have been the most disciplinary president, but since his work now appears little used, he, like Taylor, exemplifies the congenitally failing character of 'discipline'.

The occasioning of 'task' is evident when MacIver's address is compared with the disciplinary writing in two of his widely used textbooks (MacIver 1921; MacIver and Page 1950); the first appeared before his move from Toronto to Columbia, while in successive editions the second was for years a staple of American sociology. MacIver used 'task' in both, in references to evolutionary differentiation, to the division of labour, and to Durkheim's 'clever study' of it (e.g. MacIver 1921: 4; MacIver and Page 1950: 527), but in neither a Taylorite technical sense nor as a stylistic mannerism; the word rarely occurred otherwise. Although MacIver and Page (1950: 5) did talk of their own 'task' in defining terms, the tone of the two texts is more consistent with the 'science' suggested by their titles than with the transcendent connotations of 'task'.

MacIver's presidential defence of sociology as a useful science, where he used 'task' repeatedly, has a quite different tone. This passage exemplifies his use of what thus appears to be an occasioned term:

We are the scientific fiduciaries of a great enterprise. Are we big enough for the job ? Do we realise its greatness and our responsibility ? Are we gearing ourselves to it as best we can ? Or do we spend too much time disputing over little things or empty things ? Do we vex ourselves overmuch with methodological quarrels and ignore the major tasks to which our methods should be applied ? Have we enough to show for our diligent and often expensive researching ? What do we do with the piles of data we collect ? Are we asking significant questions and seeking significant answers ? Is there some danger that we sprawl over half the universe of knowledge and do not concentrate on our proper and

¹ Albeit not by name; Bacon was 'the first great philosopher of science'.

urgent business ? I would bring to your attention some of these neglected tasks.

Recalling less an epideictic after-dinner speech than a sermon from what MacIver's son-in-law described as the "joyless and oppressive Calvinism" of his upbringing (Bierstedt 1980: 82), those questions beat cumulatively upon the reader. 'Task' is clearly crucial to this erotematic tocsin.

Accompanying the purifying diagnoses of 'danger' and 'neglect', the overriding sense of MacIver's 'task' is of collective duty in a calling. Thus sociologists could be contrasted with writers in other disciplines:

They have their own missions and we have ours. Ours is to establish, by sustained investigation and interpretation, a coherent body of knowledge in which the primary relations of man to man and of man to his groups will be revealed with amplitude of perception in the clear perspective of science. Has any science a grander task ? Who shall deny its urgency ?

This 'mission' was cast in explicitly religious terms. In discussing 'social images' - or 'idols' in Bacon's sense - MacIver held that:

... Some of the priests who guard these images are bold enough to tell us that myth and fable are better than truth, that we live by our sacred illusions, that the darkness is preferable to the light. Against that faith stands the faith of science, that in the end truth serves mankind better than does falsehood or ignorance. Perhaps we social scientists can profess a further article of that simple creed, affirming that it is the dark and distorted images which nations have created that are a main cause of the present plight of our civilization. At least, we must raise the question whether the time is not fully ripe for the turning of the light of science on our sanctified prejudices, cherished delusions, and obsolete traditions. What science can do in this regard is not to evaluate our social images but to bring them into closer correspondence with social realities. It is a tremendous task.

Glossed as a 'task', the 'faith' and 'creed' of 'science' are presented with all the richness of the imagery of 'light' in the Christian tradition. But if sociology entails a *fiat lux*, it is also a Platonic move from the shadows and an enlightenment. By turning the light of science on sanctified prejudices MacIver then used religious imagery to attack religion. As for the 'dark and distorted images which nations have created' and 'the present plight of our civilization', the Scottish-born MacIver wrote when Britain was at war. He used that crisis to link science and usefulness:

Our misrepresentations, our distorted images, have increasingly become forces to tear the world asunder. If sociologists set themselves steadily to the task of investigating these images, they would be bringing science and human life together at the place where they are most apart, at the place where the separation of them appears, in this age of ours, to be most perilous. It is a progressive task that must be renewed, generation after generation. In some areas we dare not yet attempt it...

Given his somewhat archaic 'perilous' and his somewhat startling 'dare', MacIver casts sociology as a pilgrim progressing in itself and as conjointly bringing progress to an endangered world. He drew here on more than Bacon's 'idols'; scientific redemption also entailed a Baconian sense of construction:

Times of crisis reveal the role of our social images, but they reveal something more. They reveal the nature of social cohesion. On this knowledge must our science be built. To advance it is our primary task. Perhaps we are skirmishing too much along the edges of it and attacking too little the center. ...the things we need to know to build the foundations of our science are also the things men need to know if they are to find some redemption from the blindness of their impulses, from the trampling herd spirit that so often overpowers them, from the evil omens of their distorted social images.

The opportunities of the times were specifically sociological. Knowledge of social cohesion entailed 'vast areas unpossessed by us and unexplored, though our own flag and no other flies over them'. Like any colonial power, sociology would face difficulties - '[t]hese great unexplored areas offer us no easy conquest' - but would have to take up the disciplinary burden: '[l]et us get on with the job'.

In the contrast between the styles of his disciplined and disciplining languages, and in his use in particular of 'task', MacIver then both suggests the generic specificity of the term and gives the breadth of its evocations. All of the images from Chapter 8, except 'familial blood', can be found in the passages quoted above. ('Blood' in another sense is implied, in MacIver's topically military metaphors). Sociology may be a potentially useful science, but it is also a religious mission, a matter of hard constructive work, a business to be owned and developed, and a territorial imperative. MacIver's ethic and his spirit of sociology then exemplify the metaphorical and intertextual disciplining by which disciplinarity is sustained. Through his use of Bacon, he also exemplifies the effect of this process as a tacit sociology of knowledge.

MacIver has himself been tacitly present throughout the thesis, since Merton's (1936a) distinction of 'civilisation' from 'culture' was in part derived from his. This binary recurs variously in the address. MacIver distinguished, for example, two forms of knowledge, 'illumination' and 'skill', and two forms of social images:

Some ... are representations, generally skewed by our interests and emotions, but nevertheless accepted representations of existent things. Others are non-representative expressions of our dynamic values [which] do not mirror, even distortedly, any evidential datum or system of data that the scientist can investigate. They are in that sense wholly subjective ...

That epistemological divide is not conducive to a sociology of knowledge. Against the

linking of values to existential conditions in, say, Scheler's and Mannheim's axiomatic intersubjectivity, MacIver quarantined the individual from the start. So although Bacon's (1620) account of the idols of the tribe, den, marketplace and theatre was a proto-sociology of knowledge, although MacIver drew so heavily on it in his address, although as a leading user of 'knowledge' he implicitly linked sociological knowledge to the conditions of its production, and although his constructive aim of 'bringing science and human life together' was so consonant with Mannheim's, it is not surprising that he did not couch his address in subdisciplinary terms. That was consistent with his work elsewhere, for he was among Mannheim's leading American critics. Following von Schelting's (1936) epistemological critique,² he objected to the 'genetic fallacy' - that the conditions under which an argument was proposed had a bearing on its validity - and, as so often, to Mannheim's treatment of the intellectuals. MacIver (1938: 815) used Bacon to attack Mannheim's "somewhat puzzling and wholly dangerous" solution to the difficulties of his relationism: "[o]ne might suggest that insofar as the intelligentsia detaches itself from the idols of the tribe, it is not by setting up and worshipping a greater composite idol". This is itself somewhat puzzling; since the idols of the tribe are those which Bacon attributed to human nature, they are hardly relevant to Mannheim's thesis of a 'dynamic synthesis' of locationally derived perspectives. In both that critique and in his address, then, MacIver epitomised the familiar denial of Mannheim's problematic.

When combined with his standing as the most 'disciplinary' president, that double exemplification makes MacIver another limit-case for this thesis. Since the same point is reached from his epistemological assumptions and in his presidentially Baconian defence of a Baconian imaginary, he typifies the blunted reflexivity found in the sociologies of knowledge and in the situated debarment on situationally required examination of disciplinary knowledge in the addresses. His explicit denial of the subdisciplinary matched his implicit avoidance of formal reflexivity. Negatively in his own usage, positively in this reading of him, he enacts the antimetabolic argument that sociologies of knowledge are representations of an epistemologically warranted discipline, and that representatives of the discipline act as sociologists of knowledge. His use of 'task' - and as seen in the previous chapter his usage was not idiosyncratic - allowed, simultaneously, a bridge over his epistemological great divide and a

² Kettler and Meja (1995: 243) talk of his "obvious reliance on von Schelting's coaching".

fulfilment of occasioned requirements.

"Enemy of fuzzy confused thinking, champion of clarity and precision" MacIver may have been, and "the clear demonstration that good English, literary style, clarity of expression and systematic presentation are not incompatible with sociological writing" may have been among his contributions to the discipline (Alpert 1954: 287, 292), but at first glance it is difficult to see precision in his missionary depiction of sociology. The mixed metaphors in his justification of sociology as scientific and useful suggest anything but 'systematic presentation'. But if it is read as a tacit sociology of knowledge, the address has a topical coherence beyond its tropical fuzziness. MacIver's reaching forward to the discipline's 'progressive task' through a reaching back to the 'obsolete traditions' which he ostensibly condemned, for example, is less a contradiction than a resolution of the circularities of the sociologies of knowledge and of the situated limits to situated reflexivity in presidential addresses. His 'reflections on sociology during a crisis' might then be neither the bad philosophy nor bad sociology which it first appears, but a good address. Whether it was or was not a successful address is another question, and one beyond this thesis. But in relation to MacIver's career it does illustrate the contingency of disciplining invocations of disciplinarity.

MacIver may have been the most 'disciplinary' president, but, like Taylor (1946), he exemplifies the 'failing' character of 'discipline'. His writing now appears little used, and even far closer to his time in the disciplinary sun one of his former students granted that his work "has not been uniformly appreciated by his American colleagues" (Alpert 1954: 292). In Westie's (1973) survey of 'disciplinary survival', MacIver ranked equal twenty-sixth among the sixty-three presidents up to and including 1972, but since recognition of his name must have been inflated by his and Page's textbook and by his having endowed a prize in sociological theory, that appears an over-estimate of the use of his work. Indeed, although he held a central place in American sociology, as successor to Giddings at Columbia and as sponsor there of Lynd and Merton, Turner and Turner (1990: 80, 130) stress his 'foreignness', to both the country and the discipline.³ MacIver made the same point himself:

I regarded, and still regard, [sociology] as a great and challenging subject. I fought lone battles to get it accepted in Scotland and in Canada. Yet I was never happy with my accomplishments in that field. My own books in sociology did not give me anything

³ Mullins (1973), however, listed him among the 'important standard American sociologists'.

like the degree of satisfaction I got from my books in political science... I was generally out of line with the prevailing notions and doctrines of American sociology... (quoted in Bramson 1970: 13).

Although it would then be misleading in one sense to extrapolate to 'the discipline' from MacIver's standing as the most 'disciplinary' president, it is well justified in terms of fractal ordering. MacIver's enrolment of Bacon was paradigmatic of disciplinarity, his use of 'task' was the same for disciplining, and now he has been seen to typify the contingent outcome of their interaction; his place in 'the discipline' enacted the failing definitive of 'discipline'.

As seen in the following chapter, that is more generally the case.

CHAPTER 9 : FOLK DISCIPLINE

Any aspect of sociology in which disciplinarity, disciplining, and discipline are systematically represented can itself be represented as a sociology of disciplinary knowledge, and presidential addresses to the ASS/A are systematic in their rhetorical occasioning. Those are the grounds for the argument that, as representatives of 'the discipline', the presidents are folk sociologists of disciplinary knowledge. The 'folk disciplinarity' and 'folk disciplining' in the addresses having been shown in the two previous chapters, the argument is now concluded through 'folk discipline'.

'Discipline' was modelled as a contingent network of documents, devices and drilled bodies. Against the 'perennial optimism' of 'disciplinarity', it is 'congenitally failing', through susceptibility to disruption at any of its component links. Since 'the discipline' was shown to be consistent with that fractal ordering of 'discipline', but since it was also seen that 'failing' was no bar to its success, that definition is more difficult to operationalise than were those of 'disciplinarity' or 'disciplining'. As seen in the correspondence-analytic confirmation of the salience of 'discipline' - success in the analysis was claimed for the failure to achieve a neater ordering; failure in 'the discipline' was claimed on the basis of successful ordering - 'failing' exemplifies the circularities of any sociology of knowledge. Since this study is itself a fractal ordering, no Archimedean point can be claimed for it, and critique in its terms is inadequate. If strictly logical coherence is not expected, its absence cannot be called a defect. 'Failing' is not a property of disciplinary texts in themselves. Rather, the fate of any text is decided by its users, and coherence, success, is a readers' accomplishment. That was suggested in the case-studies of Taylor {1946} and MacIver {1940}, for their work now appears to be little used. But since canons are unstable, and since any forgotten author is open to re-creation - 'unjust neglect' is an established trope - such judgements are never more than provisional. Once rationality and disciplinary survival are bracketted as criteria, an alternative basis for 'failing' is required.

The means here follow from the argument that disciplined knowledge is modern knowledge, and that modern knowledge derives from a separation of the political and epistemological senses of representation. Since the foreclosure on circularity implied in that distinction, characteristic of the sociologies of knowledge, is equivalent to the situated blunting of situationally required reflexivity in presidential addresses, the

same peculiar effects should be found in them as in the formal sub-/metadiscipline. On those expectations, the chapter will be developed in three phases. To establish the link with formal study of disciplinary knowledge and to set the context for what follows, the presidents' uses of 'discipline', 'modern', and 'knowledge' will be shown in the first. Usage of each of these key terms shows the dispersiveness familiar from the two previous chapters, and a preliminary sense of 'failing' is found in that instability. Secondly, and on the definition of a discipline as a network of documents, devices and drilled bodies, the presidents' explicit responses to it will be treated: disciplined knowledge will be presented as a failing effect of writing, organisation, and community. Thirdly, the presidents have available a range of implicit responses to the aporias of modern knowledge, three of which will be illustrated: sociology is a 'young science'; both sociology and the objects of its study entail 'paradox'; and separation of the epistemological from the political is tacitly elided in sociology as 'representation'. Those three phases amount to a layered depiction of 'discipline' as 'failing'.

'Folk discipline' then involves the effects of foreclosure expected from the formal sociologies of knowledge. After that has been summarised, the argument of Part III will be recapitulated: through the disciplinarity, disciplining, and discipline in their addresses, the presidents of the ASS/A, as representatives of the discipline, can be seen as sociologists of knowledge.

DISCIPLINE, MODERN, KNOWLEDGE

Disciplined knowledge is modern knowledge; modern knowledge is disciplined knowledge. The presidents' deployment of those key terms will be used in this section to confirm that their addresses are folk sociologies of knowledge, to demonstrate the 'congenitally failing' character of 'discipline', and to set the context for a reading of the presidents' explicit and implicit responses to failure. The terms' recurrence in the addresses, and, with the exception of 'modern', their salience, has already been shown quantitatively; although none was used heavily, each of 'discipline' and 'knowledge' was a leading element in two dimensions of the correspondence-analysis. That in itself was taken as a warrant for treatment of the addresses as sociologies of knowledge. It will now be shown that each of the three is used with the dispersion, 'failure', stressed in the previous chapter. This then sets a context for a reading of the presidents' own

explicit diagnoses of 'the discipline' as failing and for their implicit resolutions of it.

Discipline

After the sociologies of knowledge were read in Part I as representations of discipline, they were linked to American sociology through Gans' (1990: 446) use of 'the discipline' as a taken-for-granted descriptor. The presidents confirm that usage, and in a pattern suggestive of an increasing common sense. Only two of the thirteen using 'the discipline' did so before 1973, and only three of the twelve instances of the related 'our (own) discipline' fell before 1970. Since earlier presidents referred unambiguously to sociology as a discipline - the term could not have been prominent in the correspondence-analysis were it temporally limited - this does not show growing concern with sociology itself, but it does index the place of 'discipline' in its folk understanding. Formal and colloquial usages coincide. 'Discipline' in the addresses has the tension found in the sociologies of knowledge as well, for while it shows an occasioned recursivity, it is also fractally dispersive.

The fact that 'discipline' emerged as a strategic term despite relatively sparse use in the addresses exemplifies in itself the congenitally failing character attributed to it. Since no president defined this central term, its effects rested more on taken-for-grantedness than on potentially distancing specificity. Usage shows the topical shift noted in 'science' in Chapter 7 and the trans-scalar effectivity of 'task' in Chapter 8. Two presidents - Thomas {1952} on 'interdisciplinary research' and Gans {1988} on 'the discipline and the public' - took it as an explicit topic, while others used it uncertainly between topic and resource. In the second sense, the presidents implied prepositionally separable referents. As might be expected, they most commonly meant by it either sociology in particular or an academic domain in general. That is the discipline *of* sociology. At a finer scale, they invoked processes *within* sociology. Thus Chapin {1935} called for well-defined concepts in place of 'undisciplined impressions'; Stouffer {1953} described the 'readiness to see' as 'due to expectation engendered and disciplined by prior theory'; and Lieberman {1991} saw sociology as distinguished by 'a disciplined approach' to evidence. More finely still, Thomas' {1952} and Gans' {1988} analyses of 'discipline' exemplify its use *as* sociology. That layering - of an outcome, of a range of processes leading to it, and of the ensemble of the two as a topic - is enough to warrant treatment of the addresses through 'discipline'. But recursion also entails dispersion.

'The discipline' is embedded in the disciplinary.

For the presidents also use 'discipline' in contexts *beyond* sociology. It is used of individuals, as when Parsons (1949) spoke of 'the disciplined organization and longer-run goals of the personality' or Alice Rossi (1983) of parental discipline. Or it denotes large-scale order, formal and informal, as in Angell's (1951) contention that the United Nations 'could not now successfully discipline one of the Great Powers', in Yinger's (1977) proposal that counter-cultural values may sap 'painfully won disciplines', or in Wilson's (1990) account of 'the disciplined habits associated with stable or steady employment'. Given the spread of referents of 'discipline' beyond sociology, 'the discipline' is continuous with more general disciplinary processes.

Whether or not that implies a Foucauldian disciplinary society, this central term in the celebrating, policing and exemplifying of sociology does have an apparently self-negating polysemy. If no word can be used without its intertextuality being evoked, then the prepositional layering of 'discipline' belies the specificity the presidents assumed for 'the discipline'. Fractally recursive and dispersive, 'discipline' has all the fuzziness of common sense. A similar instability - or failing - is found in another of the key terms of sociology: 'modern'.

Modern

Sociology is implicated in the processes studied within it. That puzzle was recast in Part I in a description of sociology as a modern discipline of the modern, and foreclosure on that circularity through separation of the political and epistemological senses of representation has been used to link the formal sociologies of knowledge and the folk version in the addresses. There is no doubt over sociology's identification with modernity: in Comte's three stages, in Marx and Engels' 'all that is solid melts into air', in Durkheim's organic and mechanical solidarities, in Toennies' *Gemeinschaft* and *Gesellschaft*, or in Weber's traditional, charismatic, and rational-legal authorities, the modern as the domain of sociology is conventionally contrasted with the traditional. The presidents assumed that antithesis. When Komarovsky (1973, emphasis added), for example, depicted 'men who were egalitarian in behavior but traditional in ideology' as showing a '*discrepant* combination of 'modern' behavior and traditional ideology', she was on orthodox ground. But orthodoxy becomes unstable when uses of 'modern' and 'tradition' are examined in detail. Around three-quarters of

the presidents used each, with a frequency similar to that of 'discipline', and their referents show the essential tension expected from fractal ordering rather than any sharpness of disciplinary location.

Although 'modern' recurs throughout the addresses as an explicit or implicit marker of change, this central sociological term cannot be periodised consistently. The presidents drew or implied contrasts from any time over the last seven hundred years. Ellsworth Faris {1937}, for example, distinguished the modern from the mediaeval:

Ours is a modern world of movement; that older day was a world of immobility. ... It is not demonstrable that the modern world is better or happier than the medieval world; it is clear, however, that it is different, very different, vastly different. There has been a transition from the changeless to change; from the absolute to the relative; from the transcendental to the human. To the modern man, truth is to be carved out by human effort, not accepted on the word of a learned doctor.

Coleman {1992} implied a similar sense when he dated the 'modern corporation' from legal changes in the thirteenth century. Or, the line might be drawn in the Renaissance or the Reformation. Wirth {1947} suggested the former, when he said that limited use of force 'has been the practice at least of modern dictators ever since Machiavelli offered his counsel to the dictators of the day', and Sanderson {1942} the latter, when he claimed that '[m]odern democracy in the Christian church began with the Protestant reformation as a revolt against ecclesiastical corruption'. The cut is more commonly made a century later, when 'modern' is identified with 'science'. Thus Merton {1957} spoke of 'the last three centuries in which modern science developed', Lazarsfeld {1962} held that the origins of empirical social research 'lay in early modern Europe (it may be dated as far back as the Seventeenth Century)', and Hauser {1968} referred to 'the three centuries of the modern era'. Or again, the modern may date from the eighteenth and nineteenth centuries, as when Lipset {1993} recalled his and Rokkan's analysis of 'modern political divisions in Europe as outgrowths of ... the National Revolution and the Industrial Revolution'. Bendix {1970} implied a similar dating when he spoke of 'the attack on the value of academic scholarship which the great critics of modern civilization launched during the 19th century'. Lazarsfeld {1962} set a transition early in that century: the 'idea of the modern university began with the Berlin seminar'. Bendix {1970} also had an alternative: '[s]ince the later 19th century modern art has been characterized increasingly by a retreat from meaning and coherence'. Or, the modern may have originated in the twentieth century, as in Short's {1984} account of the analysis of 'risk':

the modern field of risk analysis is very young... The relationship of scientific institutions to the federal government has been critical in this development. The 'modern' phase of that relationship began - as did so many federal-private arrangements - with the depression of the 1930s.

Bleeding into use of 'modern' as a synonym for 'contemporary' (a sense also given by each president quoted above), that resolves the otherwise tautological 'modern sociology' or 'modern sociologists' used by several presidents. The location of sociology in modernity appears to be agreed, so long as the meaning of modernity is not. A similar instability is found in use of 'tradition'.

The distinction between the modern and the traditional recurs throughout the addresses. Encapsulating the conventional binaries of the move, Hauser (1968) held that a 'social morphological revolution' had generated:

secondary group, as distinguished from primary group association; interpersonal relations based on utility from emotion and sentiment; the conjugal or nuclear, from the extended family; formal from informal social control; rational from traditional behavior, enacted from crevice institutions; and bureaucracy from small-scale and informal organization.

Having those associations, 'tradition' was a generalised other for the 'modern' of sociological attention. By definition resistant to dating, it could nonetheless be used to imply historical ruptures. Wirth (1947) affords a limit-case of such fuzzy specificity:

a number of changes have ... occurred since the days of the primitive local and isolated group life of our ancestors which have profoundly affected the force of tradition. The movements of populations and the contact between people from the ends of the earth, the opening of world markets, and the spread of modern technology, the growth of cities, the operation of mass media of communication, the increasing literacy of the masses of people over all the world, have combined to disintegrate local cohesion and to bring hitherto disparate and parochial cultures into contact with each other.

On that reading, the ancestors living a 'primitive local and isolated group life' might be located at any time from the palaeolithic to the invention of television. The same general contrast is found in the references to Ogburn's 'cultural lag'. Hauser (1968), for example, invoked it in his depiction of 'traditional' organised religion 'as a living museum of cultural atavisms adding to the confusion and disorder of contemporary life'.

But the traditional also denoted a preferred order, as in Kimball Young's (1945) apparently oxymoronic defence of 'our older individualistic tradition' against growth of the state, or in Bendix's (1970) of 'the Enlightenment tradition' against the 'distrust of reason' of the late 1960s. More generally, Sanderson (1942) claimed as a strength that '[s]ociological analysis has shown the power of custom and tradition', and Short (1984)

{1984} invoked Perrow's 'social rationality', which 'recognises the values placed on social ties, traditions, human dignity, and the desire for security and predictability'.

Yinger {1977} used this ambiguity interpretively when he held that 'countercultures':

do not simply contradict, they also express the situation from which they emerge - pushing away from it, deploring its contradictions, caricaturing its weaknesses and drawing on its neglected and underground traditions.

The modern, liberal and rational, is in tension with the polymorphously conservative traditional. That same effect appears when sociology itself is taken as a tradition.

For all that Parsons {1949} could claim of American sociologists that '[o]ur very lack of traditionalism perhaps makes it in some ways easier for us than for some others to delve deeply into the mysteries of how human action in society ticks', around half of the presidents who used 'tradition' did so in speaking of the discipline itself. Parsons himself was no exception, since he described the claimed convergence between Weber, Durkheim, Pareto, Marshall and Freud as a:

a very encouraging beginning, of a process of coalescence of these types of more or less explicit theory which were really integrated importantly with research, into a more general theoretical tradition of some sophistication, really the tradition of a working professional group.

Although disciplinary distinctiveness was blurred in such usage' - Goode {1972}, for example, held that sociologists had neglected the study of force 'because we share a long humanistic tradition whose biases deny the ultimate importance of force' - 'the discipline' was presented with all its features. It had 'founding fathers' {Vance 1944}, or 'forefathers' {Kohn 1987}. The 'ancestry' of terms in it 'is legitimate and honorable' {Hauser 1968}. It has a 'birthright' {Parsons 1949; Coser 1975}. Or the 'wisdom not to look elsewhere but ourselves and our discipline for [our] mandate ... is our inheritance and that so far is what we have to bequeath' {Goffman 1982}. Any disciplinary trend could be located in what Bendix {1970} called 'the creative interplay of the traditions that have formed us': Davis {1959}, for example invoked 'the traditional interest of sociologists in systematic theory', or Turner {1969} 'the tradition of Simmel, Von Wiese and Park and Burgess'. Sociology's usefulness might be presented in similar terms, as when Moore {1966} held that the 'older intellectual traditions of sociology favored the use of current knowledge for remodeling as well as merely describing the social order', Peter Rossi {1980} that 'applied social research has been a firm and fruitful part of the tradition of our discipline', and Gans {1988} that 'American sociology began in part as social criticism, and ... a handful of sociologists have continued this tradition'.

Toward the end of the period of this study, presidents from the earlier years were listed as traditional, as in Riley's {1986} 'traditions of Sorokin or Parsons, if not of Comte'. So too was work from that time, in claims to both continuity and discontinuity. When Peter Rossi {1980} held that a 'very respectable, quite traditional theoretical strain in criminology links together poverty and crime', or Erikson {1985} that analysts of alienation at work 'begin, as sociologists traditionally have, with the standard indices of dissatisfaction', they referred to studies from the 1930s and 1940s. Work continued from that period might also be used antithetically, as when Komarovsky {1973} departed from 'traditional role analysis', and Goffman {1982} from 'social roles in the traditional sense'. Here again is 'tradition' as 'other'. The sociological tradition then appears to be as polyvalently contestable as the traditional more generally, a tension caught nicely in Whyte's {1981} departure from 'traditional explanations of peasant traditionalism and resistance to change'.

Both 'modern' and 'tradition' are moveable feasts, and in ways which belie the conventional distinction between them. 'Tradition', of course, is by definition trans-temporal, but 'modern' has an at least tacitly orthodox dating within sociology, and this was only fuzzily supported. 'Tradition' in the addresses ran from pre-history, and 'modern' from the mediaeval, and each culminated in the present. Each was an exigent resource. The presidents were modern critics and defenders of modernity and traditional critics and defenders of tradition, evincing then the circularity of the sociologies of knowledge. Insofar as sociology is a modern discipline of the modern, and insofar as the modern is not the traditional, the 'failing' of discipline then lies not so much in the collective oxymorons of the presidents' usage as in the omission of resolution of them. 'The modern' and 'the traditional' could be taken for granted only as long as they were not reconciled. One effect of that is an uncertainty over sociological 'knowledge'.

Knowledge

A reading of presidential addresses as a folk subdiscipline requires that the presidents be shown as concerned with sociological 'knowledge'. Since it was a leading term in two dimensions of the correspondence-analysis, and since the concern was evident in the passages quoted in Chapters 7 and 8, this does not need elaborate review. One point is made briefly: that the presidents distinguished disciplined sociological knowledge

from common sense knowledge, in a contrast leading inevitably to 'failure'.

As seen under 'disciplinarity', the presidents widely assumed that sociological knowledge was, relatedly, both scientific and politically consequential. That meant they needed to separate it from, and articulate it to, popular common sense. As Bendix (1970) said in defending 'reasoned inquiry' in sociology, 'commitment to scientific work makes sense if there is hope that in the long run the constructive uses of knowledge will prevail'. But problems in linking sociological knowledge to policy were noted in Chapter 7, while the simultaneous denial of the political in imagined scientificity and its reinsertion in constructive usefulness left the particularity of sociological knowledge uncertain. It was warranted as *not* common sense. Hankins (1938), for example, held that '[i]nstead of knowledge, ignorance often becomes the basis of hope.... we know that the popular mind is necessarily guided less by knowledge than by emotions based on interests and a scheme of values'. Conversely, sociologists' sharing of such schemes was frequently stressed. Wirth's (1947) version is typical:

The student of society will be plagued by the difficulties of achieving 'objectivity', by the existence of social values, by the competition with common sense knowledge, by the limits of his freedom and capacity to experiment, and by other serious and peculiar handicaps which trouble the natural scientist less or not at all.

Such competition was complicated by common sense beliefs about science itself. While that might be recognised - as Donald Young (1955) said, '[p]opular expectation needs be regarded as simply a condition that sociologists ... must take into account' - it was far from clear what it entailed. Disciplined knowledge and common sense knowledge were interfused. Davis (1959), for example, noted the way that 'sociologists, working mainly on their own society, often take for granted the broad knowledge and interpretation (the 'theory') of this society'. But where he deplored that interpenetration, others welcomed it, as when Gans (1988), echoing Taylor's (1946) call for a reconciliation of sociology and common sense, held that 'lay people do not label their knowledge about society sociology, but nonetheless it consists of ideas and data in all of the fields we study'. The relation between sociological and common sense knowledges might then be couched in terms of an objective distance or of a quasi-hermeneutic circling. But those alternatives lead to complementary difficulties: sociological distinctiveness requires a dream of objectivity; acceptance of disciplinary embeddedness implies a loss of distinctiveness. On either count, the standing of sociological knowledge is uncertain.

So while 'knowledge' has been shown to be a concern within the addresses, its

matching with disciplinarity guarantees the intractability of problems in its ordering.

Discipline, modern, knowledge: Reprise

'Discipline', 'modern', and 'knowledge' are key terms in the readings of both the formal sociologies of knowledge and presidential addresses. Their salience has been illustrated in this section, but so too has the instability of their use. Found to be fractally recursive and dispersive, 'discipline' located sociology uncertainly in wider disciplinary patterns. Sociology was 'modern', so long as the term could be applied across three-quarters of a millennium, and as a 'tradition' constituted around separation from the traditional. Sociological 'knowledge' was shown to be just as imbricated with common sense. Those orderings confirm that the addresses can be read as sociologies of knowledge, they demonstrate in themselves the 'congenitally failing' character of 'discipline', and they set the context for the examination in the next two sections of the presidents' explicit and implicit recognitions of 'failing'.

DISCIPLINED KNOWLEDGE

Sociological knowledge is disciplined knowledge, and disciplined knowledge has been seen as problematic. Supplementing the 'disciplining' of the previous chapter, this section is devoted to the image of sociology emerging from the presidents' explicit recognition of 'failure'. After a note on their diagnostic language, it will be developed through the 'documents, devices, and drilled bodies' comprising 'the discipline'.

The presidents variously recognised disciplinary failure. They contrasted 'chaos', for example, with the 'order' of sociological attention.¹ Thus Lundberg {1943} referred to work in sociology as 'the chaos of unrelated projects, large and small, from doctor's theses to the gaudier studies supported by the foundations', and Blumer {1956} to 'the rather chaotic condition that prevails in the selection of variables'. They used 'crisis',² as when Bendix {1970} saw sociology as entangled in a more general 'crisis of legitimacy' or Coser {1975} diagnosed 'crisis and fatigue within the discipline'. Or, they invoked 'failure' itself. To mention, say, the presidents who took 'race' as their topic, Frazier {1948} claimed that 'we have failed to study race contacts as a phase of men's behavior as members of groups', Hughes {1963} that 'we failed to foresee present

¹ No president used 'chaos' in the sense of 'chaos theory'.

² This was not applied to sociology until its Kuhnian sense had become a commonplace.

racial movements because our whole inward frame is adapted to study of the middle range of behavior, with occasional conducted tours toward, but not dangerously near, the extremes',³ and Wilson {1990} that '[m]any current studies of poverty ... fail to make [the] connection' between culture and social structure.

But responses to failing discipline go beyond these intimations, to be more generally patternable by the model of a discipline as a network of 'documents, devices, and drilled bodies'. The presidents' accounts of problems in disciplinary language will be treated here under 'documents', their remarks on faults in formal organisation of sociology under 'devices', and their warnings over loss of community under 'drilled bodies'. These amount to a depiction of 'the discipline' as permanently failing.

Documents - Rhetoric, language, writing

Disciplined knowledge is written knowledge. Kuhn's inclusion of a specialised language in his disciplinary matrix is axiomatic to this thesis, as is the treatment of language through rhetoric. The first was validated in the content-analysis, where the 'vocabulary of sociology' was fitted to accounts of 'the discipline', and rhetoric has also been matched with the addresses. The presidents' own allowances for these processes is then of interest. It will be shown here that while use of a disciplinary language is one thing, acceptance of the disciplinary as language is quite another, for the presidents' remarks on rhetoric, language and writing both make explicit the failing of discipline and guarantee its continuation.

'Language' is typical of the topics of the addresses. Only Gamson {1994} treated it at length, in showing the place of language in 'the politics of exclusion'; thus refusal to use gender-inclusive language 'sends the same underlying message to women in the 1990s as the sign "No Irish Need Apply" sent to Irish immigrants in the 1850s'. But as resource rather than as topic it appeared in any number of addresses. Hughes' {1963} account of linguistic 'cleansing', for example, had prefigured Gamson. When Lazarsfeld {1962} spoke of difficulties in integrating 'research institutes' in universities by holding that 'academic tradition favors the rhetoric of facilitation while the inner dynamics of such institutes press towards increasing autonomy and self-direction', or when Bendix {1970} discussed 'the rhetorical use of the term "fascism"', each implied the rhetorical constitutivity assumed in this thesis. Gans {1988} made that explicit when he quoted

³ This is an unusual case of one president criticising another, and even it was tacit.

Gusfield's (1976: 17) claim that 'scientific work is rhetorical' and that it is so 'even in its stylistic appeal to a rhetoric of not having a rhetoric'. But the presidents more usually used 'rhetoric' in its pejorative sense, as when Hauser (1968) introduced his 'social morphological revolution' with an apology 'for injecting this abominable rhetoric into the literature', or as in Lee's (1976) critique of scientism:

To the extent that we have approached [pure science] we have contributed chiefly to the complexities of the rhetorical and statistical games so many academicians and other researchers now play as pretentious substitutes for trying to perceive and understand both social constructions of 'reality' and whatever lies behind or stimulates those constructions.

Language in general was treated just as ambiguously. It might be held as crucial to the discipline, as in Parsons' (1949) claim that his 'general theory' could 'help to provide a common language to facilitate communication between workers in different branches of the field', in Coser's (1975) description of ethnomethodology as a 'language disease', or in Short's (1984) noting of the fragmenting effect of the way that 'when new fields and subfields develop, they quickly breed their own technical languages'. The linking of language and disciplinary pathologies recurred, and often with the suggestion that disciplined sociology entailed a distance from language. Thus Chapin (1935) held that:

All social theory must rely heavily upon concepts. In the early stages of formulation, purely verbal concepts that describe undisciplined impressions naturally play a major role. But if social theory is to advance beyond the dangers of verbalism of emotion and attain the stability given by objective experience, it is necessary that a larger and larger proportion of social concepts be operationally defined.

Other accounts were more florid. Queen (1941) described a 'mechanism of sociological flight from reality usually involv[ing] verbalization often carried to the point of mass production'. Holding that sociologists needed 'a record of demonstrated superiority to the word mongers whose magic today formulates public policy and enchants the public', Lundberg (1943) queried 'the nature and limitations of our most ancient and traditional tools, namely, the words of the language that have come down to us from pre-scientific times'. Davis (1959) warned against 'the easy path of verbal tapestry', and Lee (1976) dismissed much sociology as 'dilettantish verbal fretworks'.

The implication that sociology as science could be distinguished from sociology as language entailed a revisiting of the boundary between sociology and common sense. This is evident in Davis' (1959) diagnosis of confusion over the relationships described by functionalists:

Misunderstanding in this matter seems to arise from two sources: first from the language used in describing the relationships, second from the special problems of applying systematic analysis to human societies. As to language, if the investigator uses phrases

like 'has the function of', 'meets the need of', or simply 'is for', the words have so many connotations and ambiguities that the effect is often to obstruct rather than to facilitate the conveyance of meaning. Part of the reason is that these are words borrowed from common discourse and hence mainly used to indicate moral imperatives and volitional intent rather than sheer causal relationships. ... It is of course extraordinarily difficult to escape from such words...

Homans [1964] also treated functionalists' language, but to different ends. He held that problems would be solved were 'theory' to be understood properly, for that was what distinguished sociology as science from common sense:

What the functionalists actually produced was not a theory but a new language for describing social structure ... much of the work they called theoretical consisted in showing how the words in other languages, including that of everyday life, could be translated into theirs. They would say, for instance, that what other people called making a living was called in their language goal-attainment. But what makes a theory is deduction, not translation.

Or for Lieberman [1991], scientific sociology required valid evidence more than theory.

This too entailed a distance from the practices by which common sense was invoked:

sociology exists because of its attitude toward evidence, to wit, a disciplined approach that attempts to use the standards of scientific inquiry in estimating truth. Sociology is not the only pathway to understanding society. A good novel or a good journalistic analysis can do much, but they rest on different standards. A work of fiction, for example, or even an essay, depends on the reader resonating to the writer's personal position. Proof lies in the agreement between reader and writer, and the latter is free to use whatever literary devices are available to create that feeling and understanding. Keep in mind that there is much to be said for our standards of evidence.

The imagining in all this of a realm of facticity beyond language is one element of the 'scientific' moment of disciplinarity. Difficult to sustain in itself - Davis' problems of connotation, ambiguity, and common sense are, of course, opportunities in this reading of the addresses - it also conflicts with 'usefulness'. For sociology to be useful it had to be presented through precisely the resonance which Lieberman denied. Otherwise, as

Chapin [1935] said, sociologists were limited to:

articles of a scholarly sort which probably never come to the attention of the public administrator or the business leader because they are tucked away in journals unknown to the public and couched in technical language unintelligible to most readers.

Although Gans [1988] spoke of the 'lay public' rather than of 'administrators and leaders', he made a similar point in a call for greater disciplinary recognition of 'public sociologists'. These were able 'to discuss even sociological concepts and theories in the English of the college-educated reader, probably because they enjoy writing as well as doing research ... their work is intellectual as well as scientific'. But this still implies a distinction between sociology and writing, in that first (perhaps ironical) 'even' and in the (apparently not ironical) contrast between 'scientific' and 'intellectual'. Even

when it is queried, the imaginary of science entails an imaginary of neutral language.

The 'documents' of sociology are then one form of failing discipline, for when they discussed or dismissed the language of sociology the presidents identified an abiding difficulty. Their approach to it also ensured its persistence. In the blunted reflexivity of the addresses, even those who stressed or implied the constitutivity of language did not treat sociological knowledge in those terms. A similar effect is found in the 'device' of formal organisation.

Devices - Organisation

Disciplined knowledge is organised knowledge. Such formal devices as codes of ethics, agreements on curricula and training, the maintenance of journals, or national organisation distinguish the disciplinary from more general common sense. To take only the last of these, the discipline's formal organisation was another focus for the presidents' own diagnoses of failure. This is consistent with the history of American sociology, for it was seen that formal arrangements were never quite right; but responses to centripetal and centrifugal strains in the ASS/A have been said to have blurred its disciplinary focus (Simpson and Simpson 1994; Huber 1995). The formal organisation of the discipline was then both a symptom of congenital failure and another guarantee of its continuation.

A practice of the early ASS - that each annual meeting had a unifying theme - affords a glimpse of the process by which organisation was adapted, with uncertain success, to disciplinary shifts. Ellsworth Faris {1937} talked of a change introduced during his presidency:

About fifteen years ago recognition was given to the increasing differentiation of interests in the specialized efforts of our members and the formation of divisions and sections began, a practice that has continued with increasing emphasis. This year another step has been taken which seems to have met with wide-spread approval in omitting entirely any designation of a central theme for the program of the annual meeting.

But since 'diversification has reached such a point that the annual meetings should perhaps try to review common denominators one by one', Lazarsfeld {1962} reintroduced the convention of an annual theme. That standard practice is one element definitive of the presidency, and the annual theme is a matter of disciplinary unity. As Riley {1986} said of hers and of 'my talk as President':

I propose to link sociology to the recent development of one sociological field, the sociology of age. ... As with other special fields, I believe that an understanding of age can clarify and specify time-honored sociological propositions, raise new research questions, demand new

(as well as the old) methodological approaches, and even enhance the integrative power of our discipline (a power eroded in recent years through pluralism and disputes).

The settling of disputes, or the reconciliation of the new and the old, the specific and the general, was recurrently depicted as a question of formal organisation.

As might be expected from the finding that the presidents' organisational 'embeddedness' peaked in the golden age, that theme is most evident in the addresses during the years preceding and following it. In the former, for example, Queen (1941) claimed that '[w]ith our diversified interests and many semi-independent sections we have almost ceased to attempt integration'. While he granted that other groupings of sociologists had a place, he held organisational centrality as an ideal:

We will divide our support in accordance with our abilities but in no case will we permit national unity to suffer. ... We may support some specialized and some regional organizations, but we will not permit them to interfere with the growth of the American Sociological Society.

Sanderson (1942) saw such a focus as crucial to both sociology's mission and sociologists' practical opportunities:

Sociology has made and will increasingly make a large contribution to the growth of democracy. We as sociologists have a responsibility for enlarging its influence to this end. To do this we must greatly strengthen this Society, our national professional organization, so that it may advance sociology effectively. ... Membership in this Society should be a prerequisite for professional employment. ...

Lundberg (1943) made a similar claim, although, more concerned with sociology as science than with sociology as useful, he qualified Sanderson's last point:

The American Sociological Society has thus far been unable to define a professional sociologist. Supposedly he would be a person possessed of special skill and training, as contrasted with anyone else who also has six dollars and spends it as dues for the Society.

Now whether or not professional closure was achieved, the ASS did become the more professional ASA, a point which Lee (1976), a would-be reformer, raised in his address. Seeing professionalisation as entailing internal and external exclusiveness, he claimed to have brought to his presidency the intent 'to do what I could to push the ASA toward a more representative performance'. But he also recalled Sanderson's pragmatic stress, for one of his goals was 'to broaden our occupational opportunities'. Peter Rossi's (1980) diagnosis of the failings of formal organisation entailed a similarly dual emphasis:

Our professional association currently has an academic bias that manifests itself in a variety of ways ... The American Sociological Association should be taking the lead in helping to bridge the distance between our discipline and the intellectual and employment opportunities in applied social research.

Or Gans (1988) pointed to organisational inadequacy in the bridging of distances within

sociology itself; since 'our identity as sociologists' was 'social as well as intellectual',

we should ... look at social mechanisms that can contribute to being and feeling a part of a single discipline. ASA does what it can along these lines, but only a bare majority of all sociologists belong, and much too small a number of them are involved in ASA as other than receivers of its services. Also, the organization still relies excessively for its agenda and leadership on academics from the major research universities to be fully representative.

But Gans exemplified what he identified. His distinction between 'intellectual' and 'social', or of the political from the epistemological, implied a permanent instability.

Insofar as that distinction is embedded in formal organisation, insofar, that is, as disciplined knowledge is treated as epiphenomenal rather than as an effect of disciplinary devices, the failing diagnosed by the presidents is itself constitutive of sociology. It cannot be otherwise. Just as critiques of disciplinary writing were written, the presidents' critiques of disciplinary organisation are organisational outcomes, and as long as that circularity is not included it is as tacitly disruptive as open reflexivity would be. Another version of it appears when 'documents' and 'devices' are augmented with the community of 'drilled bodies'.

Drilled bodies - Community

Disciplined knowledge is collective knowledge. As in any formal organisation, the discipline is also a matter of informal and affective coherence, and the presidents often diagnosed its troubles as a failing in 'community'. The trope was well-established before Kuhn's work was absorbed in the discipline. Writing a year after Parsons' claims for general theory, for example, Cottrell [1950] had tacitly queried his optimism:

It is one thing to claim a notable advance in general orientation and theory; it is another to say that a desirable level of precision and articulation in theoretical formulation and in method has been achieved ... Notwithstanding some rather reckless promises made by some in the heat of seeking commercial and government research contracts, a candid appraisal must find much of our terminology extremely fuzzy, our hypotheses lacking in rigorous casting and our methods as yet not well adapted for operationally testing our hypotheses or for yielding that consensual validation of observation upon which any community of scientists must rely for recognizing what is accepted fact.

That sense of disciplinary 'community' implied a unity of sameness, comprising bodies drilled in a *conscience collectif*.

As might be expected from the history of 'the discipline', concern over unity is rather more evident in the latter part of the period of the study. At the height of the golden age, Williams (1958) had noted that 'the proliferation of numerous specialized fields, each with its many specific studies, has created pressing problems of coherence

and order', and diagnoses of communal disorder became increasingly common thereafter. Sorokin (1965), for example, held that 'a vast number of strikingly different and often discordant currents of sociological thought have emerged and proliferated in modern sociology', while Bendix (1970) and Coser (1975) both used the image of a 'scientific community' under threat from within. Lee (1976) attributed dispersion to sources of funding, for sociologists were caught in the:

competition and conflict of interest groups within which they live and work. Despite their frequent anxiety to preserve their scientific status distinct from that of 'politicians' and from that of 'mere journalists' for the mass media, they are quite mindful of issues uppermost in the mass media and thus in the decision-making processes of grant donors. ... we have a variety of sociologies born of diverse definitions of social situations.

Blalock (1979), finding that 'we seem badly divided into a myriad of theoretical and methodological schools that tend to oversimplify each other's positions... and that encourage partisan attacks', and warning of 'the inherent dangers of endless theoretical and methodological debates and a further fractionating of our field', called for sociologists 'to work together' in 'a concerted effort' in which there 'will still be plenty of room for differences'. In a similar vein, Lieberman (1991) deplored the 'non-productive paradigm battles' in sociology. Short (1984) noted the intractability of the problem:

So varied are the methods and perspectives of sociological analysis that the discipline lacks a central focus. The discipline appears to be confused and chaotic, perhaps because of this lack, yet there is disagreement as to the need for, or the proper substance of, a central focus.

Riley (1986) was more optimistic. Suggesting that 'sociology's greatest strength [was] its integrative power', and that '[i]n recent decades this power has frequently been obscured - in pluralism, parochialism, destructive polemics, extremes of individualistic versus sociologistic explanations, criticisms from within and from without', she still looked to 'an era of reintegration'. If Gans (1988) was more resigned than hopeful - 'reintegration is neither likely nor desirable in a pluralistic discipline' - he likewise sought mechanisms 'to bring us together as specialization moves us ever further apart'. But whether the presidents responded in hope or in despair, they did agree that the disciplinary community was failing.

Those accounts of drilled bodies breaking ranks are as self-exemplifying as the presidents' remarks on language and organisation. But here the effect was less a blunting of reflexivity than a contradictory selectivity from the sociological tradition. Rather than looking to the agonism of Durkheimian unity through difference, Marxist dialectic, or Simmelian conflict, the presidents treated the discipline through all the

binary pathos of lost *Gemeinschaft* which has also been constitutive of sociology since its emergence. Even Coser (1975), in his rare 'exercise in the uses of controversy', fell back on 'community'. Once again, the presidents' responses to the discipline's failings assured their continuation.

Disciplined knowledge: Reprise

Disciplined knowledge has been treated as an effect of networks of documents, devices, and drilled bodies. Under each head the presidents were found to address the congenitally failing character of 'discipline'. 'Documents' were equated with disciplinary writing. The presidents may have deplored either the closeness to everyday discourse of sociological language or a distance from it, but they often treated its failings. Formal organisation was taken as a disciplinary 'device'. Again, whether the presidents diagnosed over- or under-organisation, this facet of the discipline was seen recurrently as failing. 'Drilled bodies', finally, were treated through images of disciplinary (comm)unity, and here too claims of fragmentation were common. Each set of responses was also found to be self-exemplifying; recognition of the discipline as failing was routinely couched in terms which guaranteed that failure would continue.

That occurs in the presidents' explicit responses. Now, just as 'tropical disciplining' allowed a practical resolution of their situated blunting of a situationally required reflexivity, so they have means of implicitly accommodating the recurrence of failure. These are seen when disciplined knowledge is treated as modern knowledge.

MODERN KNOWLEDGE

Disciplined knowledge is modern knowledge. That identification from Part I has been confirmed in this chapter. The presidents did diagnose the failings expected from the modelling of 'discipline, but while that was another step towards treatment of the addresses as sociology of knowledge, it is hardly startling: as Sewell (1971) said, '[i]t is a sociological truism that great gaps often exist between stated goals and their implementation'. But it is also to be expected that the presidents had means of bridging those gaps, or of reconciling their optimistic claims for sociology with their recognitions of failure. Three such are treated here, selected for their resonance with motifs from

Part I. The presidents excused sociology's shortcomings through its standing as a 'young science'; they implied, that is, the "romance with 'ends' and 'beginnings'" which Crook (1991: 218) found in sociological accounts of modernity. They found 'paradoxes' in either what they studied or how they did so; the referent here is the question-begging in responses to 'Mannheim's paradox'. Or they used 'represent' in eliding their distinction of the epistemological from the political; this refers to Latour's (1991) understanding of modernity. The failing character of discipline is evident in each.

Young science

Sociology is the Peter Pan of disciplines. At least since Comte (1830-42 II: 57) spoke of the 'infantile state of social science', sociology has been depicted as a 'young science', immature but full of promise, always just on the verge of growing up. Like any child, it can be forgiven its failings, for these are markers of progress and signs of the achievement to follow. Each major movement in the discipline has had a rhetoric of maturity within reach. Now, at last, this time, real progress will be made; now that failings have been recognised, sociology can come of age; now, finally, real growth is possible. These familiar tropes recur in the addresses.

In his address on sociologists' neglect of 'business as an institution', Fairchild (1936) used the image of 'young science' to explain disciplinary boundaries:

By the time sociology began tardily to mature itself into a self-respecting scientific discipline the structure of economics had become so well integrated, and had achieved for itself such a large and well merited measure of social recognition and esteem that the new science naturally felt reluctant to invade the field.

One element is missing here, a standard against which the youth of sociology could be judged. Ellsworth Faris (1937) provided that the following year:

if we have not come as far as we could have hoped, we can take comfort in the fact that we have not been at our task very long as compared with the physical sciences. It was 144 years from the discovery of the revolution of the heavenly orbs to the writing of the mathematical formula that explained their movements; 144 years from Copernicus to Newton. Give us 144 years and we ought to have, by that time, comparable results !

Lundberg (1943) similarly held that '[i]t took scientists generations to achieve their present status. We must expect to follow their rough road'. Parsons (1949) elaborated the image - 'I do not think it fair to say that we are still in the stage of proto-science. But we are unquestionably in that of a distinctly *immature* science' - and his colleagues, students, and those working within the orthodox consensus most repeated it thereafter. Stouffer (1953), for example, defended the tentativeness of his work on the grounds that

'[t]he demand for experimental proof is rather young in Western culture... It is still younger in fields which are closer to sociology and social psychology'. Or Donald Young (1955) ascribed the scantiness of links between sociology and the 'practicing professions' to, *inter alia*, the fact that it 'is a youthful discipline and has a correspondingly small store of useful knowledge'. As when Coser (1975) held that sociology 'is not advanced enough to rely on precisely measured variables', or Riley (1986) that the study of age 'can help dramatize the sociological perspective and stimulate its utilization... [b]ut the task is only beginning', the connotations lingered. These could be reaffirmed even as the image was deprecated. Williams (1958), for example, claimed that:

The degree of intellectual control we can now command over an incomparably difficult body of complex phenomena is of an order ... that needs no apology and requires little defense. Perhaps the time has come to de-emphasize the youthfulness of sociology in favour of inventorying its progress toward the responsible exercise of an adult role in the community of sciences.

But he also saw 'merit in regarding the effective history of a discipline of sociology having authentically scientific aspirations as, for the most part, extending back rather less than a century'. Now, at last, this time, the 'adult role' appears to be within sight, and it is an effect of the 'authentically scientific'. To be sure, those who either tacitly or directly questioned the image were also from the orthodox consensus. When Moore (1966) said 'Whether one dates sociology from Plato or from Comte...', or when Goode (1972) noted that '[f]or two millennia, social analysts have given much attention to [force]', each allowed a dating of sociology which belied youthfulness.

Now 'young science' is but one among many organic tropes for both sociology and the social. The presidents amply support Brown's (1977) location of the organic among the discipline's 'root metaphors'; half of the forty-six who used 'grow', for example, did so with reference to sociology itself. Within that cluster of images, 'young science' has a dual effect. 'Young' means 'alive', 'alive' means 'growth', and, as Williams (1958) implied, 'growth' means that central image of modernity, 'progress'. That in turn is a matter of the 'authentically scientific'. But 'young science' also excuses the failure to achieve it. Being nearly, almost, not quite ready for its more measured adult role, a young science can be forgiven its fractiousness and failings.

The image is then well suited to a discipline falling short of the optimism of disciplinarity. But where it implies that the discipline is not yet ready for the world, the presidents' use of 'paradox' suggests a world out of kilter with the discipline.

Paradox

'Mannheim's paradox was a point of departure in Part I, where the sociologies of knowledge were read by a contrast between binary denial and trinary acceptance of the problems of self-exemplification. It was seen that since a finding of 'paradox' required an assumption of the epistemological coherence which Mannheim had queried, his critics required the very circularity with which they charged him. The dream of it being possible to be otherwise is the dream of modernity, once that is defined as a separation of political and epistemological senses of representation. To find 'paradox' is then to grant failure, but to shift it from account to phenomenon.

The presidents' use of 'paradox' is characteristically polyvalent. It appears most simply in the sense of a Kuhnian 'anomaly'. Merton (1957), for example, evidenced his reading of 'priority' by 'the seeming paradox that even ... meek and unaggressive men, ordinarily slow to press their own claims in other spheres of life, will often do so in their scientific work'. Although he did not mention Kuhn, Lieberman (1991) made that sense explicit: the 'evidence addressing a given theory is likely to be inconsistent... This means that we have to interact with the data to resolve apparent paradoxes'. A response to paradox in that sense is relatively straightforward; theory can be adjusted to resolve it, as Merton did with 'priority'. The same effect arises when 'paradox' denotes 'contradiction'. Goode (1972) exemplified that usage:

[T]he importance of force and force-threat in human behavior is richly demonstrated by the rarity of its use ... [E]very social system is a force system. The two statements do not make a paradox, because every social system contains mechanisms, processes, and patterns whose result and often intention is to prevent the outbreak of overt force.

Again, that is amenable to relatively simple resolution, even when, as in Moore's (1966) noting that the model of social evolution 'has had the paradoxical effect of inhibiting attention to the future', the theoretical overhaul has to be drastic. Usage becomes more complicated when 'paradox' is applied not to theory but to what is being theorised.

When Loomis (1967) reported that '[t]he paradox of the traditional thriving right alongside the modern was common', he said more about his own assumptions than about the India he was describing. Distinguished from the traditional, the modern is a benchmark of rationality, and one form of rationality is assumed. That sense, of course, is well-established in the sociological tradition. Blau (1974), for example, evoked Durkheim when he argued that structural differentiation 'brings about macrosocial integration, paradoxical as this seems, inasmuch as differentiation is conceptualized as

restricting social intercourse and integration as contingent on it'. Or Short {1984}

recalled Weber's 'fate of our times'; it was:

characterized by the 'disenchantment of the world'. Paradoxically, because knowledge is increasingly esoteric and in important ways incomplete, its discovery and applications are often accompanied by mystification, misunderstanding, and impatience and disillusionment with what is known.

The tradition might be so taken for granted that departures from it were peculiar. That was the sense of Goffman's {1982} noting that his claim that body-to-body interaction was 'the primordial real thing' implied:

paradoxically ... that a very central sociological distinction may not be initially relevant: namely, the standard contrast between village life and city life, between domestic settings and public ones, between intimate, long-standing relations and fleeting impersonal ones.

At that breaching of constitutive binaries, the imbrication of sociology in the modern becomes explicitly problematic, and the presidents' usage comes closest to the sense of 'Mannheim's paradox'.

When sociological rationality does not fit the world, the presidents often locate failure in the latter, quarantining rationality itself. That appears, for example, in Hankins' {1938} 'paradox of collective social action':

Fully rational action is impossible for a group as a whole, not only because one cannot clearly foresee the outcome of a plan or policy, but also because action by the collectivity is dependent on stirring up the sentiments and emotions of the group members.

Or when Bendix {1970} referred to 'the paradox of the development I have sketched', in his defence of the 'reasoned inquiry' of the Enlightenment, he invoked the same taken-for-grantedness, taking the questioning of the sufficiency of rationality as 'a retreat from meaning and coherence', and as evidence of 'an ethics of social despair'. A similar sense, finally, is evident in Davis' {1959} worrying at the language of functionalism:

'function', 'disfunction', 'latent', 'needs' are treacherous for the same reason that they are handy ... they are susceptible of easy expansion by knitting together ready-made intuitions, connotations, and ambiguities. But for this reason they are strikingly inappropriate for doing the opposite of moralistic reasoning - that is, for explaining in a detached manner the moral and religious ideas and behavior of mankind. It is this paradox that lies behind many charges against functionalism.

The dream of a language beyond ambiguity is, again, the dream of modern rationality. It is also a guarantee of failure. But to take its effects as 'paradoxes' is to shift the locus of failure from the disciplined to what escapes dualistic disciplining, and in just the same move as in diagnoses of 'Mannheim's paradox'.

Now, Davis of course could invoke that imaginary only through language which was itself connotative and ambiguous. 'Represent' is a prime example of it.

Represent

'Representation' links the formal and folk poles of this thesis. Latour's (1991) identification of the modern with the separation of political and epistemological representations was the basis for the rhetorical reading of the sociologies of knowledge in Part I, and Goffman's (1982) description of the presidents of academic associations being 'led to feel that they are representative of something, and that this something is just what their intellectual community wants represented and needs representing' was one warrant for the reading of presidential addresses. The presidents' use of 'represent' and its derivatives is then of interest. Despite being used comparably with 'discipline' and 'knowledge', 'represent', like 'modern', was not significant in the correspondence-analysis. But where it could not be decided then whether that absence was due to irrelevance or to taken-for-grantedness, the salience of the latter can now be shown. While 'represent' is as dispersive as the rest of the presidents' language, the commonest usage suggests a simultaneous division between and elision of its epistemological and political senses. It is another glossing of failure. That further sign of 'folk discipline' can be extended. Since 'represent' is a virtual synonym of Mannheim's 'documentary', it allows the tacit linkage of knowledge and existential conditions expected from a folk sociology of knowledge. If 'paradox' recalls denial of the sociology of knowledge, 'represent' recalls its practice.

The word appears in a pictorial sense, as when Becker (1960) noted that beings seen as evil 'are often represented as having tails, cloven hooves, horns, and other animal attributes'. It is used technically of democratic representation - from Queen's (1940) 'representatives of the American Peace Society' to Lipset's (1993) discussion of 'proportional representation'. It is also used technically of sampling, from Taylor's (1946) selection of counties to 'represent the rural life of seven generalized major type-of-farming regions' to Coleman's (1992) 'nationally representative sample'. The epistemological effect of the fusion of those ways of 'standing for' was occasionally made explicit. In his treatment of 'social images', for example, MacIver (1940), held that some 'are representations, generally skewed by our interests and emotions, but nevertheless accepted representations of existent things'. Or Chapin (1935) claimed that 'sound theory':

scrutinizes the discrepancies between levels of symbols: levels that begin with the the more concrete symbolic substitutes for social reality and ascend to the more abstract symbolic substitutes. It examines such systems to insure that the transitions from level to level are

logically made and do not depart from representations of reality to a degree that creates invalidating errors.

If Moore {1966} was rather less sanguine about 'reality' - '[n]o analytic science could dispense with models that are in some way or some degree unrealistic representations of the phenomena that they are designed to put in order' - he still echoed Chapin's stress on the political character of analysis: to represent is to 'put in order'.

Davis {1959} offers a paradigmatic case of those simultaneously political and epistemological effects. Still referring to the language of functionalism, he held that 'there is no reason to eliminate the basic thinking represented by [its] postulates ... but there is reason to phrase them differently and in terms less open to logical confusion and ideological attack'. In this one passage he epitomises a standard move, the difficulties to which it gives rise, and an alternative to it. His purifying distinctions between the logical and the ideological and between 'thinking' and 'terms' amount to the separation of the epistemological from the political; the linguistic cleansing of his 'eliminate' is the inevitably tacit reinsertion of the latter into the former; his 'represented' implies a practice at odds with the original move. Davis' use suggests both that access to the 'essential nature' or 'ethos' of an object which Mannheim (1921-2) claimed for the circling between parts and whole in his documentary method and that sense of the 'anticipatorily knowable' which Garfinkel (1967: 40) attributed to it. It suggests, that is, the pervasion of the epistemological by the always already political. That sense of 'standing for' is by far the most common usage in the addresses.

The ambivalently Mannheimian Wirth {1947} hinted at its effectivity in a tacit sociology of knowledge: '[p]ublic opinion precipitated through the clash of representative ideas reflect[s] more or less faithfully the positions confronting the respective groups that compose the society'. His conjunction of 'represent' and 'reflect' recalls Mills' (1939) and Merton's (1945) point, that the sociology of knowledge was characterised by imprecision in the linking of 'thought' and 'society'; such typically used terms as 'correspond', 'reflect', 'express', or 'determine', they held, elided the difficulties of establishing connections. Once their crucial assumption is bracketted - that *analytical* precision is both desirable and attainable - their case can be adapted. If the formal sociology of knowledge depends on lay terms, then the use of lay terms for the relation between knowledge and context suggests a folk sociology of knowledge. As used in the addresses, 'represent' is such a word. Fractally dispersive, and tacitly disruptive of the distinction between the epistemological and the political, it appears

with three interpenetrative emphases: *as* sociology and *of* politics; *in* sociology; and *of* sociology *as* politics.

The first sense is evident from the earliest addresses to the latest. It appears in Chapin's {1935} claim that '[t]he Social Security Act represents a compromise between many forces each driving towards ends that different leaders regarded as socially-desirable goals', and in Coleman's {1992} diagnosis that 'evacuation of the household ... represents a social change that can be described as the decline of primordial institutions and their slow replacement by constructed organizations'. Each of these entails an unproblematised mapping from politico-social events to sociological findings. This is 'represent' *as* sociology and *of* politics.

The politically epistemological recurs when 'represent' is applied to practices within sociology itself, being evident across the gamut of disciplinary moments. It is found in sociological theory, as in Komarovsky's {1973} 'hunch that the psychological assumptions generally made by sociologists represent not psychological constants, but contingent propositions'. It appears in accounts of sociological findings, as when Sorokin {1965} maintained that much research of his time 'represents mainly a reiteration, variation, refinement and verification of the methods and theories developed by sociologists of the preceding period'. It recurs in defence of sociology's usefulness, as in Hauser's {1968} claim that his 'social engineering' 'represents an utterly new approach to contemporary problems'. Or it is used of the discipline as organised, as when Gans {1988} held that the 'ASR, being the flagship journal, is supposed to represent the best in sociology'. Usage across these referents gives 'represent' *in* sociology, and, again, in such a way as to imply an anticipatorily knowable common sense.

Or the word might be used to make that folk sociology of knowledge all but explicit. That is the sense of Williams' {1958} suggestion that the 'proliferation of particular topics' within the discipline:

represents an entirely normal division of labor and specialization of skills and knowledge. To what extent the division of labor represents the 'organic solidarity' we have a right to expect of a coherent field of study is itself a subject for sociological analysis.

Analysis of one division of labour among many would require the linking of disciplinary knowledge to the contexts of its production. This was posed, if not developed, by some presidents. Frazier {1948}, for example, held that 'the sociological definition of the problem [of race] represented a rationalization of the social attitudes of the class in the white community from which sociologists were recruited'. Or when Lee {1976} charged

that 'individuals and groups put sociology together representatively', he meant that sociologists stood for the 'narrow interests of society's ruling elites'. 'Represent' in this sense is *of* sociology, *as* politics.

Those three emphases are separable prepositionally, but not in effectivity. Reconciliation in each of them of the epistemological and the political entails a fuzzy commonality rather than any analytical distinctiveness. The presidents' polysemic use of 'represent' is then a marker of the failure of 'discipline', in allowing tacit reassertion of what is rationally denied. That one fit between the addresses and fractal ordering of the disciplinary is matched by another, at a larger scale. Insofar as the presidents' quasi-documentary usage is Mannheimian, 'represent' confirms that the representatives of the discipline are folk sociologists of disciplinary knowledge. That point will be taken up later in the chapter. For the moment, it is enough that 'represent' has been shown as characteristic of modern knowledge.

Modern knowledge: Reprise

As disciplined knowledge, sociological knowledge is modern knowledge. If the modern is an effect of the separation of the epistemological and the political, the presidents were expected to have means of accommodating the failures to which this gives rise. Three mechanisms have been shown. The presidents excused the shortcomings of sociology on the grounds that it was a 'young science', no matter how ancient the traditions from which it is constructed; taking rationality as a given rather than as the emergent achievement of their own practice, they attributed to 'paradox' its inevitable inadequacy; and they tacitly belied their distinctions of the political from the epistemological in their use of 'represent'. Each device recurs throughout the addresses. Each shows a different facet of the failing character of discipline. Each is both a response to and a re-enactment of the peculiarities of modern knowledge.

FOLK DISCIPLINE: REPRISE

In the fractal and trinitarian ordering of the disciplinary, 'discipline' was defined as a congenitally failing network of documents, devices, and drilled bodies. The fit between that model and the addresses was shown in this chapter, as the final step in a demonstration that as representatives of 'the discipline' the presidents of the ASS/A

are folk sociologists of knowledge.

On an understanding of 'failure' as the breaching of the circularity given in any study of knowledge, the argument was developed in three phases. In the first, the presidents were shown to use key terms from the formal sociologies of knowledge: 'discipline', 'modern', and 'knowledge' itself. Each was seen to be a recurrent concern, and each showed a simultaneous recursion and dispersion. The presidents did attend to 'discipline', but since it was fractally entangled in wider disciplinary networks its distinctiveness was uncertain. They did treat sociology as 'modern', but this was all but coextensive with the 'traditional' of sociology's conventional other. They did focus on sociological 'knowledge', but it was imbricated with more general common sense. That amounted to a *prima facie* case for a reading of the addresses as documentary of 'folk discipline': the presidents depicted sociological knowledge as both disciplined and modern, and in ways which suggested its failing character. Their explicit and implicit responses to that state were shown in the next two phases.

As disciplined, sociological knowledge is an effect of networks of documents, devices and drilled bodies, and the presidents diagnosed failure in each of these. They repeatedly faulted disciplinary writing and disciplinary language. They recurrently treated the device of formal disciplinary organisation as never quite right, and often enough as badly out of kilter. They routinely pointed to disciplinary fragmentation and loss of community. In each case, the terms of their readings implied the continuation of the failure they diagnosed. A similar effect is found in their implicit responses.

Just as the presidents used 'tropical disciplining' to accommodate the situated blunting of their situationally required reflexivity, they were expected to have means of continuing 'discipline' in the face of failure, and these means were expected to confirm sociological knowledge as modern knowledge. Three mechanisms were shown, in the presidents' use of 'young science', of 'paradox', and of 'represent'. Since the first entailed a traditional denial of tradition, the second a shift of the inevitable failings of strict rationality from the discipline to the objects of its study, and the third an elision of the overtly distinguished epistemological and political, these gave sociological knowledge as modern knowledge.

Those three phases amounted to a demonstration that the 'folk discipline' in the addresses matches the formal modelling of 'discipline'. The jaggedness of the matching must be stressed, for while the themes illustrated throughout the chapter

were identifiable, and while the centrality of some had a degree of numerical backing, they were themes only. Their contestability and their trans-scalar effectivity were entangled with all the other threads of the addresses. But within those limits, this reading of 'folk discipline' completes the representation of the presidents of the ASS/A as folk sociologists of knowledge.

REPRESENTATIVES OF THE DISCIPLINE AS SOCIOLOGISTS OF KNOWLEDGE : REPRISE

In Part I of the thesis, the sociologies of knowledge were read as representations of discipline, on a model of the disciplinary as an effect of disciplinarity, disciplining, and discipline: disciplinarity is a perennially optimistic and Baconian imaginary of collectivity, science and usefulness; discipline is the congenitally failing realisation of it; and disciplining is the diffuse set of practices linking the other two moments. It was claimed that any set of texts which could be matched to that fractal ordering could also be treated as a sociology of knowledge. A preliminary case for reading presidential addresses to the ASS/A in those terms was made in Part II, where the presidents were shown to be situationally required to examine, celebrate and exemplify 'the discipline', but to be situationally debarred from doing so with formal rigour. Rhetorical resolution of this rhetorical constraint set the basis for treatment of presidential addresses as a folk sociology of knowledge. After their match with fractal ordering was shown quantitatively, Part III was devoted to a rhetorical reading of them.

The common sense suffusing the disciplinary rhetorics in the addresses is a version of both the taken-for-granted binaries and the alternative trinaries found in the formal sociologies of knowledge. While each address, individually, entailed the characteristic dualisms of modernity, the ensemble of them was amenable to fractal ordering. Amid their treatments of particular topics, the presidents did invoke a 'folk disciplinarity', in that they appealed to the optimistic and Baconian imaginaries of collectivity, science, and usefulness. They did engage in 'folk disciplining', in that they drew tropical links between the order of sociology and all and any other forms and sources of social order. They did diagnose the failure expected of a 'folk discipline', in relation to concerns over 'discipline', 'knowledge', and 'modern', and in ways, moreover, which continued the failure they had found. The presidents' resolutions of generic

constraints are structurally equivalent to the responses to difficulties over rationality found in the formal sociologies of knowledge. Since the rhetorics in those sociologies and in presidential addresses are then two versions of the same fractal ordering, the antimetabolic argument of the thesis is complete: sociologies of knowledge are formal representations of discipline; presidents of the ASS/A, formal representatives of the discipline, are folk sociologists of knowledge.

CONCLUSION

Now that the antimetabole of the thesis has been established, its implications for the puzzle set in the introduction can be outlined. These will be presented in two phases.

First, Coleman's {1992} presidential manifesto for 'the rational reconstruction of society' will be read as epitomising what has been found in the addresses overall.

Then, the resolution of 'Mannheim's paradox', paradigmatic of sociology's location in the processes studied within it, will be shown through a critical summary of fractal ordering and disciplinary rhetorics.

CASE 9: COLEMAN'S MODERNITY

A review of Coleman's [1992] manifesto for 'the rational reconstruction of society' and for a 'change in the very stance of the discipline to its subject matter' allows the argument of the thesis to be recapitulated: formal sociologies of knowledge are representations of discipline; as representatives of the discipline the presidents of the ASS/A are folk sociologists of knowledge; the interpenetration of folk and formal shows the puzzling effects of sociology's location in what is studied within it, or of its standing as a modern discipline of the modern. The address is particularly convenient, being directed to the problem of the thesis - the circularity of sociology - but resting on opposed assumptions. Where it is axiomatic in the sociologies of knowledge that "[a]ll human knowledge ... is not empirical but '*a priori*' knowledge" (Scheler 1925: 67) or that "strictly speaking it is incorrect to say that the individual thinks" (Mannheim 1936a: 3), Coleman's unit of analysis is the individual maximiser of the democratic and economic theory derived from Hobbes, Locke and Rousseau. Where rationality has been treated in the thesis as emergent, Coleman took the rational as given. Where the thesis was drawn from non-foundational sociologies, Coleman drew his address from his *Foundations of Social Theory* (1990a). Since his premises are conventionally modern, and since he focussed, more explicitly than did any other president, on the oddities of a discipline which is an effect of processes described within it, his address is another limit-case in this reading of the full set; the very sharpness of his formal reading of sociology and modernity shows the folk messiness of their co-emergence.

By definition Coleman was a formal representative of 'the discipline, and so shared in the uncertainty of that representation. Whatever the justice in one obituary description of him as "the greatest sociological force this century" (Schneider 1995: 10), he was certainly among the more celebrated of the presidents in this study. That was confirmed in the analysis of the presidents' mutual referencing: a former student of Merton and Lazarsfeld, Coleman fell between them as the third most cited president. Given that the history of controversy over his work prevents any easy identification of him with the disciplinary mainstream, but that he shared the disciplinary common sense of the other presidents, he is better seen as representatively topical.¹

His address will be read here in three stages. The argument will be outlined

¹ Mullins (1973) did not count him among the 'standard American sociologists', but did list him as an important 'new causal theorist'.

His address will be read here in three stages. The argument will be outlined first. This will then be shown to be both a folk and an explicit sociology of knowledge. Coleman's example is followed in this, for he located his address in the context of his *Foundations*, and it is there that its relation to the sub-/metadiscipline is most clear; while he distinguished his work from the sociology of knowledge, a comparison with one of Mannheim's late papers shows that he repeated moves within it. This will be taken, thirdly, as epitomising Coleman's modernity. In the 'work of purification' of his separations of nature from culture and of the epistemological from the political, or in his traditional denial of the tradition, his axiomatic rationality will be seen as a rhetorical achievement. The more that he questioned the entanglement of sociology in the concerns and analytic figures of modernity, then, the more he confirmed it. That in turn can be generalised to the full set of addresses.

Coleman derived his call for the simultaneous reconstruction of sociology and society from a version of the Great Transformation. On his account, the shift from agriculture to manufacture, the concurrent urbanisation, and the transcendence of space and time enabled by electronic media had resulted in the replacement of 'primordial social institutions' by the 'purposively constructed'; 'primordial' denoted use of the family as a central element or model. Trends since the feudal era had culminated in a fundamental social transformation, since primordial institutions were so eroded that they no longer sustained normative development. Having emerged and been elaborated in response to those structural changes, sociology should now be reoriented toward the rational reconstruction required to redress their pathologies.

The three shifts were an effect, Coleman held, of the invention in law of the 'fictitious person'. First proposed in the thirteenth century, this granted an existence to institutions which was independent of their constituents. Institutions constructed on that basis allowed more simple social relationships. Whereas natural structures like the family required that each member interact personally with each of the others, constructed institutions relied only on individual 'balances of satisfaction' with the whole, with relationships among persons replaced by formalised dealings between positions. While this echoed Toennies, it was not a lament for a lost *Gemeinschaft*:

We need not mourn the loss of the supports for the social controls of primordial organization. As anyone who was raised in a small close community knows, normative systems have many unpleasant aspects. They operate more via constraints and coercion

than via incentives and rewards. They are inegalitarian, giving those with most power in the community freedoms that are denied others. They discriminate, particularly against the young, enforcing norms that are in the interest of elders; they inhibit innovation and creativity; they bring a greyness to life that dampens hope and aspiration..

Against this, constructed institutions offered the potential of individual freedom and individual fulfilment. Coleman invoked Mayoite industrial sociology to suggest a way of renewing the normative development lost with erosion of the primordial; since the continuity and closure allowing norms to be generated in familial structures were also a feature of work-places, he proposed that formal institutions be designed to make use of the informal groupings which would inevitably flourish within them.

Coleman offered a scheme for 'bounties on children' as an exemplar. Given the shift from agriculture, the move of women into the workforce, and the lengthening of children's formal education, he held that the household, the primordial locus of child-rearing, had been all but terminally eroded, and that children were then 'abandoned psychologically and socially'. Since the outlawing of child-labour meant a loss of parents' property right in children, and since aged-care was more a concern of the state than of the family, parents had no incentive to raise socially productive children. As the party with the most incentive, the state should then construct an institution which would restore to parents a form of property right. Coleman proposed that a reward, a 'bounty on children', be payable either to parents or to sub-contracted child-rearers, contingent on both the child's projected contribution to society and factors which might complicate the fostering of productivity. The state would not tell child-rearers *how* to achieve their ends; that would be left to the informal organisation sure to arise within the formal. Such a scheme implied a place for sociology.

Under new conditions the discipline had 'a future in the design of organizations, institutions and social environments', and sociologists should be trained 'to be the architects and architectural aides in the design of social institutions'. Beyond design, sociologists would be required in the administration of schemes such as 'bounties on children', since it 'would necessitate use of social science methods to make a statistical prediction ... of the expected costs and benefits to government of a given child'. Seeing such reforms as inevitable, Coleman concluded that refusal by sociologists to engage with them would ensure disciplinary marginalisation. That followed from his claim that the 'ultimate justification' of sociological knowledge lay in contributions to purposively constructed institutions.

This is a familiar move. Even if no other president put it so strongly, Coleman's address fits the folk sociology of knowledge in the full set. Furthermore, Coleman did what few other presidents attempted; he linked his account of the discipline to the formal sociologies of knowledge.

The folk sociology of knowledge in the addresses followed from the modelling of the disciplinary into the fractal and optimistic imaginary of science, usefulness, and collectivity of 'disciplinarity', into its congenitally failing realisation in 'discipline', and into 'disciplining' as the loose set of processes linking the two. The fit between the model and the address will already be evident. Indeed, Coleman all but made explicit the trinitarian interaction definitive of it:

we make a mistake, both in society and in sociology. The mistake is simple and correctable: We fail to recognize that the social capital on which primordial social organization depends is vanishing; we fail to recognize that societies of the future will be constructed, and that we should direct our attention to designing those social structures.

Optimistic disciplinarity, failing discipline, and normative disciplining are given in that one passage. Being as sanguine, as diagnostic, and as morally infused as the other addresses, Coleman's fits the model, and Coleman himself is then as representative of the Baconian strand in American sociology as any other president. The antimetabolic relation of that folk sociology of knowledge to the formal appears in his reading of the sub-/metadiscipline.

The address is an epitome of Coleman's *Foundations*. As ambitious as its title suggests, directed to the Hobbesian "question of the peaceful coexistence of man and society, as two intersecting systems of action" (Coleman 1990a: 5), and elaborated on the contractarian model of rational actors pursuing their 'interests', this book recapitulates Coleman's work over forty years. As Coleman himself stressed, that work is distinct from the sociologies of knowledge. But then an oddity arises. While of course he was far from unique among the presidents in having treated the sociology of knowledge somewhere in his work (and in fact did so far less than others) he does appear to have been alone in not only accepting its circularities but in treating them as foundational. In *Foundations*, and again in the address, he stressed that an account of disciplinary knowledge must be included in any social theory. Sociology as a discipline, he held:

came into being and grew during the period in which the constructed social environment began to grow and displace the natural social environment. This is not coincidental but is grounded in a special property of the discipline. The discipline itself, as an object of

investigation, falls within the scope of the subject matter of the discipline. That is, sociology is a reflexive discipline, whose subject matter encompasses itself. One implication of this reflexivity is that as long as the social environment is natural, the discipline cannot justify its own existence, except as an epiphenomenon, irrelevant to social functioning. It is when the change to a constructed social environment begins that sociology begins to be relevant, as an aid to that construction (Coleman 1990a: 610).

Even if his dating of sociology is peculiar, and even if the conclusion he drew from it is jarring, that call for the effects of circularity to be included in sociological writing is recognisable from the sociologies of knowledge. It is the puzzle of this thesis. Just as in the reading in Chapter 2, Coleman's (1990a: 610) solution entailed an interfusion of the epistemological and the political:

A second implication of sociology's reflexivity is that an unusual constraint is imposed on any social theory that seeks to be comprehensive. It must meet not only the two usual criteria for theory (that is, internal consistency and correspondence to reality) but also a third: The content of the theory must be such as to account for the action of engaging in the construction of social theory. If, in a theory of social change, there is no role for sociological knowledge in affecting social change, the theory cannot account for the act of theorizing itself. Theory construction in sociology must, according to such a theory, be purposeless, since it has no consequences.

Again, *mutatis mutandis*, this is familiar ground. Coleman's (1990a: 611) claims that any sociological theory "must be subjected to the test for reflexive consistency", and that to pass it "a theory must provide a location for sociological knowledge, and the activity which generates it, in the functioning of society", could be taken from Mannheim, from Barnes and Bloor, or from the actor-network theorists. If the meaning of 'location' and the implications of 'role' are left open, a problematic is agreed: the circular imbrication of disciplinary knowledge in broader political contexts. This is the apparent oddity. Far from sharing the conventional rejection, Coleman appeared to take 'Mannheim's paradox' for granted.

But he did not put it in those terms. Insofar as he mentioned the sociology of knowledge - its "extensive literature" had "contributed to the reflexive task" (Coleman 1990a: 615 n1) - it was to distance his work from it. As evident in the variation on the argument from self-refutation in his account of Marx, Coleman (1990a: 611) claimed to have identified in it an interruption of reflexivity:

... Marx was the originator of the sociology of knowledge; he presented a specific theory about the way in which conditions of social existence bring about knowledge, beliefs, and values concerning social functioning - but no theory about the way knowledge, beliefs, and values might affect social conditions.

That odd reading of praxis - ignoring Marx's critique of utopias, Coleman (1990a: 612) faulted him for not developing "ideas about how the classless society would operate" -

is symptomatic; Marx was at fault for not sharing Coleman's assumption of the primacy of the epistemological over the materially political. That assumption was the basis for his 'location' of sociological knowledge in 'social functioning'. That in turn is a move familiar from Mannheim's trajectory.

Coleman made self-exemplifyingly scant use of the sociological tradition in his individualist *Foundations*, a silence recurring in his note the sociology of knowledge. It is striking, for example, that despite a precedent in 'Mannheim's paradox', despite Mannheim's shift from the sociology of knowledge to the social engineering which Coleman espoused, and despite Coleman's (e.g. 1990b: 99) reading of his work, he was not mentioned once in the nearly one thousand pages of the *Foundations*. Mannheim's, presumably, was among the work from which Coleman distanced his own. But to depart is to return. Mannheim's (1943) call for a 'sociology of adjustment' prefigures Coleman's 'rational reconstruction'. Writing for an audience of Anglicans, he had invoked Jung's revival of Augustine's 'archetypes' and Burckhardt's 'primordial images' to revise his previously Weberian account of the 'despiritualization' of modern life: now, he said, "[i]t mainly consists in the evaporation of primordial images or archetypes which have directed the life-experience of mankind through the ages" (Mannheim 1943: 135).

Sociologists of adjustment were required first to recognise the function of these images, and then to encourage their revival. Since daily life is taylorised and devoid of dramatised completeness without them:

no consistent conduct, no character formation and no real human coexistence and cooperation are possible. Without them our universe of discourse loses its articulation, conduct falls to pieces, and only disconnected bits of successful behaviour patterns and fragments of adjustment to an ever-changing environment remain (Mannheim 1943: 136).

Continuing his earlier account of the intellectuals, Mannheim (1943: 148) had no doubt as to who was best suited to redress these pathologies:

If we fail to replace the vanishing old social controls by new ones, we can be quite sure that substitute controls will emerge, but it is very doubtful that these ... will be more adequate than those which could be provided by the cooperative thinking of the best brains among our scientists, theologians, philosophers, educationists, social workers, etc.

'Control' was too important to be left to the 'naive and unsophisticated mind' of the lay public. The 'real solution' to loss of normative cohesion required a directed resolution of 'primary group virtues' and 'primordial images' with formal organisations. Mannheim (1943: 151) held that:

In the world of large-scale institutions this means tolerating only those institutions

which either embody in their structure the principles of these primary virtues, or make it possible for these virtues to be practised in personal relationships.

In the diagnosis, in the prescription for what would be tolerated, even in the imagery, Mannheim had sketched what Coleman was to elaborate as a new foundationalism. That similarity allows Coleman to be read in the same terms as was Mannheim.

It was seen in Chapter 2 that Mannheim defended his 'sociology of adjustment' at the cost of the trinitarian tension he had sustained throughout his early work. The legislative authority he imputed to intellectuals, to sociologists in particular, and even more specifically to sociologists of knowledge, followed from his privileging of the scientific over the historicist and phenomenological moments in his methodology, and from his favouring of the rationally liberal over the conservative and socialist moments in his political grounding. Mannheim's end-point in a previously rejected and dualistic assumption of the primacy of the rational was Coleman's point of departure. That resolves what the apparent oddity in his account of disciplinary knowledge. He did not need to purify 'Mannheim's paradox'; the problem simply does not arise when rationality itself is not at issue, for the separation and re-fusion of the political and the epistemological can be managed *sub rosa*. But axiomatic debarment of the difficulty does not remove its effects. On the identity between Coleman's work and Mannheim's shift from the trinary to the binary, Coleman appears as a standard sociologist of knowledge, working within what Law (1994: 138, emphasis removed) called the "ordering strains towards dualism built into the modern project".

So Coleman can be read as a sociologist of knowledge from either pole of this thesis. His rationality is a version of the preemptions characteristic of the formal sub-/metadiscipline. His disciplinarity, disciplining, and discipline, and his collectivity, science and usefulness, yield a version of the folk sociology of knowledge of presidential addresses. Those antimetabolic exemplifications amount to an exemplification of the arational achievement of rationality.

Modernity has been used throughout the thesis in Latour's (1991) sense, as an effect of 'work of translation' and 'work of purification' - where the latter denotes the ontological distinction drawn between the 'nature' and 'culture' hybridised in the former - and to an accompanying distinction of the political from the epistemological. Now Coleman was concerned with the modern in more conventional terms; his location of the Great Transformation in the French and industrial revolutions is one version of it,

and his foundational reading of Hobbes, Locke and Rousseau is another. But his work also fits Latour's sense: the 'fictitious persons' of his constructed institutions are new Leviathans (cf. Callon and Latour 1981), which, as neither wholly natural nor wholly cultural, are effects of 'work of translation'; and the linked distinctions of the natural from the cultural and of the epistemological from the political given in the assumption of rationality are paradigmatic of 'work of purification'. That double identification makes Coleman's address particularly illustrative of achieved rationality.

Bacon was imagined in Part I as a convenient focal point in the tradition of the sociologies of knowledge. His rhetorical denial of the rhetorical achievement of science was shown as recycling a classical move and as prefiguring its recurrence in the sub-/metadiscipline, and his Salomon's House was one source of disciplinarity. While Coleman mentioned Bacon in neither his *Foundations* nor his address, he has been seen to share that Baconian imaginary with the other presidents. Now, although he did not problematise language, he will be shown as achieving rationality rhetorically. Just as in his exemplifications of both formal and folk sociologies of knowledge, that exemplifies the interfusion of the two poles of the thesis.

The rhetoric of Coleman's address is in part generic and in part characteristic of his work in general. On the understanding that they are inextricably entangled, those two effects will be shown separately here, through the genealogy of his sociology of sociological knowledge, and through the binaries of his analyses.

Perhaps surprisingly in view of his call for reflexivity, Coleman did not locate himself in the occasion of his address. But his generic adoption of occasioned demands appears in the almost explicit sociology of knowledge in his account of the discipline. It might be recalled that he saw sociology as having co-emerged with three structural shifts, and as now requiring a fourth adaptation to the 'fundamental transformation' wrought by them. His genealogy of exemplary writers in these developments is summarised in Figure 10.1.

Figure 10.1: Coleman's genealogy of society and sociology

Society	Sociology
Industrialisation	Comte, Marx, Durkheim
Urbanisation	Toennies, Park, Weber, the Lynds
Mass media, complex organisations	Merton, Lazarsfeld, Blau and Duncan
Policy research, constructed institutions	[Coleman]

While Coleman used that listing of disciplinary folk heroes analytically, it also entails an occasioned fusion of the three genres of classical rhetoric which goes beyond analysis. It is forensic, being a diagnosis of the history and state of the discipline. It is deliberative, as a setting of a natural direction to be followed. It is epideictic, in both its modest acceptance that honour was due (Coleman did not refer to his own work in policy research) and its reallocation of credit to teachers and colleagues. Those generic features have an effectivity belying Coleman's foundational assumption, since they are at once a claim to analytical rationality, an appropriate appeal to an audience, and a Kuhnian rewriting of history to justify the position being advanced. Without such a reading, Coleman's proposed reorientation of the discipline - '[t]his involves ... social theory - but social theory directed to this task, not to chronicling and conceptualizing the changes of the past' - rests on a simple contradiction. But with it, his account of the discipline is a rhetorical preemption of the essentially contestable. Management of that tension entails 'work of purification'.

As seen in the comparison with Mannheim, Coleman's oppositional rhetoric is familiar from the sociologies of knowledge. Recalling Scheler or Merton, it is a rhetoric of 'two worlds'. In one version of a distinction which recurs throughout his work, for example, between 'discipline knowledge' and 'applied knowledge', Coleman (1978: 687) held that it was 'useful':

to conceive of two worlds governed by different norms and having different properties. One is the world of the discipline, or alternatively, the world of sociological knowledge, in which sociological research is carried out and social theory is developed. The other is the world of action, in which policies are made and consequential events occur.

This echo of Scheler's ontologies of spirit and world, and of Merton's lines between civilisation and culture or discipline and profession, follows of course from treatment of the politico-normative as emergent but the rational as given. The instability of that binary is evident in Coleman's most sustained distinction, and one that bears so much of his analysis, that between 'primordial' - explicitly identified with 'natural' - and 'purposive'. This is paradigmatically purifying, requiring as it does the separation of nature and culture. But since it also implies a contrast between culture and civilisation, and since culture is then switched from one pole of the dichotomy to the other, it is also paradigmatically uncertain. Similarly, once Coleman (e.g. 1990a: 1-5) rejects *homo sociologicus* and takes the rational and 'interest'-driven individual as the "natural unit of observation", nature appears on either side of the antithesis. If purposiveness is

natural, primordial, Coleman's analysis collapses. His rationality is possible only so long as his antitheses work rhetorically.

Now since Coleman did not problematise language,² he appears to assume that representation entails a direct mapping from world to word. Rhetoric in that case would be pernicious. In following Hobbes, then, he took as implicit what Hobbes made explicit, for he, Bacon's one-time amanuensis, shared Bacon's rhetorical rejection of rhetoric.³ But if Coleman took Hobbes' problem of order, he did not take Hobbes' linguistic ordering. His organic metaphors sap his distinction between primordial and purposive. When he held, for example (emphasis added), that sociology 'came into being and *grew* during the period in which the constructed social environment began to *grow* and displace the natural social environment', both sociology and the constructed environment appear natural in their purposiveness. Or Coleman opened his address by describing a canoe-trip he took with his sons down the Wisconsin and Mississippi Rivers, depicting it as a journey through time and as a passage from the primordial to the constructed landscape. "In the beginning all the World was *America*", Locke (1689: 301) had written, and here was Coleman, Huckleberry Locke, heading downstream from Injun country, through the state of nature to a time when all the world is America again.

Although all the presidents treated or implied Coleman's themes, none did so with his sharpness and convenience for this thesis. Since no other made explicit the puzzling effects of sociology's circularities, the fact that Coleman took them as a central problematic, and on assumptions opposed to those of the thesis, makes his address a limit-case for the reading of the full set. It was found that it could be fitted to both the folk and formal sociologies of knowledge. The first fit meant that Coleman was a representative representative of the discipline: he shared the imaginary of sociology as collective, scientific, and useful; he shared the sense of its failure; and he shared the normative calls for renewal. The second gave his work as a representation of discipline, which in turn implied a traditional depiction of the traditional as lost. Since the two matchings were fractally derived and practically entangled, Coleman's address epitomises the argument that the presidents are folk articulators of the formal.

² cf. Coleman's (1992: 264) refusal to grant Stinchcombe's (1992: 188-90) point on gender-specific language.

³ In listing sources of "Absurd conclusions", for example, Hobbes (1651: 114) included "the use of Metaphors, Tropes, and other Rhetoricall figures, in stead of words proper".

That was shown in the rhetorical maintenance of Coleman's rationality, for his assumption that rationality was given but that normativity required explanation, or his distinction between epistemological and political representations, were necessarily belied in their elaboration. His 'discipline knowledge' appears to be as constructionist as his 'applied knowledge'. That breach of the reflexivity for which Coleman called is of less interest as a contradiction as such, than as an illustration of the ineluctability of analytical contradiction. That is one process implied in fractal ordering, and in the very sharpness of his defence of an alternative Coleman exemplified it.

CHAPTER 10 : DISCIPLINARY RHETORICS AND FRACTAL ORDERING

Sociological knowledge is an ordering puzzle. Its foundations are uncertain, its authority is contested, and the self-exemplification required by its location in processes described within it leads to the silencing effects of 'Mannheim's paradox'. The fractal ordering of disciplinary rhetorics in this thesis is a practical resolution of that puzzle. Derived from, and applied to, two genres in which disciplinary knowledge is at issue - the sociologies of knowledge and presidential addresses to the American Sociological Society/Association - it describes a jagged and trans-scalar self-similarity in formal and folk accounts. The overall argument of the thesis will be briefly retraced here, before its implications are suggested and its limits stressed.

In a prefiguring of the tension between binary and trinary orderings sustained throughout, the study rested on three postulates and was structured antimetabolically. Those postulates were: that 'discipline' is a paradigmatic figure of sociology's self-location; that the cognitively modern is an effect of the dualisms epitomised in the separation of political from epistemological representation; and that representation is always rhetorically inflected. The thesis then comprised an examination of disciplinary rhetorics in both the formal sociologies of knowledge and the informal versions of them deployed in the peculiar genre of presidential addresses. In its central conceit, these were read antimetabolically rather than antithetically: formal sociologies of knowledge are representations of discipline, and the addresses delivered by formal representatives of the discipline are folk sociologies of knowledge.

Two modes of ordering recurred in a reading of the sociologies of knowledge for rhetorical diagnoses of 'Mannheim's paradox'. The first entailed dualistic preemptions of its circularities. In Scheler's and Mannheim's sociologies of knowledge, in Merton's and Kuhn's sociologies of science, in Bloor's and Barnes' 'strong programme' and in Mulkay's 'discourse analysis', these preemptions invoked various dyadic correlates to separation of the political and the epistemological. Prominent examples include base/superstructure, world/spirit, real/ideal factors, social/ technical norms, civilisation/culture, rhetorics of hope/rhetorics of fear, or interpretative/empiricist

repertoires. Whether these dyads were used in defence or critique of sociological foundations, their form alone set a continuity of foreclosure.

A second, trinary, alternative to such ordering also typified the field. It was suggested by Scheler, elaborated in Mannheim's triadic architectonic, and repeated in Kuhn's 'disciplinary matrix'. It recurred in the actor-network theory used to guide the reading: in Law's (1986b) description of networks as effects of documents, devices and drilled bodies, and in Latour's (1991) account of them as at once real like nature, narrated like discourse, and collective like society. This trinitarianism allows an interactive, 'continually receding', avoidance of binary foreclosures. Since trinary categories are suggested rhetorically as exhaustive but are not mutually exclusive, strict analysis with them is precluded; the best that can be expected are Law's (1994: 107) "fairly coherent ... ordering patterns". One homology between Mannheim's, Kuhn's and Latour's work was used to describe fairly coherent regularities in treatments of 'discipline' and in methods for the study of disciplinary knowledge.

Each of these regularities was amenable to trinary patterning. To summarise studies of 'discipline', the disciplinary was modelled as an effect of disciplinarity, disciplining, and discipline, with disciplinarity denoting a Baconian, optimistic and material imaginary of science, usefulness and collectivity, discipline a perennially failing network of documents, devices and docile bodies, and disciplining the practices of the other two moments' mutual renewal. Methods used in the study of knowledge were similarly ordered. Dispersions in accounts of disciplinary knowledge as scientifically real, rhetorically discursive, or textually ethnographic, were resolved in a recurrent pattern of the non-arbitrarily contingent. That led to a Kuhnian 'preferred analogy' for description of the sociologies of knowledge as representations of discipline: 'fractal ordering'. This metaphor of disciplinary rhetorics was intended to allow for the bounded contingency of trinary ordering, for the jagged self-similarity of its trans-scalar and trans-historical recurrence, and for the requirement for self-exemplification. The basis for comparison of formal and folk sociologies of knowledge was drawn from it; any set of texts in which knowledge is at issue and which is open to fractal ordering is at least a tacit sociology of knowledge. One such set is found in presidential addresses to the American Sociological Society/Association.

The place of presidential addresses in American sociology, 'the discipline', was suggested in Part II. The presidents are, formally, 'representative of something', but

once 'the discipline' was shown to match the modelling of 'discipline' as congenitally failing, that 'something' is elusive. American sociology is at once a science, a profession and a moral community, with all the bounded contingency of their interaction. That is what makes presidential addresses a peculiar genre. Living 'Mannheim's paradox', the presidents are generically required to police, exemplify, and celebrate disciplinary knowledge but are generically debarred from doing so with formal rigour. Constrained by the need to appeal to the disciplinary common sense of a motley audience, their addresses can be read as a folk sociology of knowledge. It was shown quantitatively that these representatives of 'the discipline' could indeed be treated as sociologists of knowledge - although generic features accounted for only 40% of the variability in the addresses - and that 'discipline' and 'knowledge' were among their central concerns. Such a reading was presented in Part III.

The addresses are structurally equivalent to the sociologies of knowledge. The rhetorical resolution in them of occasioned constraints leads to the same effects as does the sociologising of epistemology in the sub-/metadiscipline; they are pervaded by the same separation of the political from the authoritatively knowable, and by the same problematic re-fusion. This was shown in a patterning of the presidents' appeals to disciplinary common sense by a folk disciplinarity, a folk disciplining, and a folk sense of discipline. The requirement for epideictic, forensic, and deliberative claims on the audience ensured that the addresses' performativity was topical and tropical, rather than strictly rational. The relatively natural disciplinary worldview invoked by the presidents then merged with both that of the formal sociologies of knowledge - even down to a use of constructive imagery consistent with recent constructionist versions - and that of the world at large.

That was the result expected from the antimetabolic structuring of the thesis - sociologies of knowledge are formal representations of discipline; representatives of 'the discipline' are folk sociologists of knowledge - and that is the process of 'fractal ordering'. Since the formal sociologies of knowledge are reflexive representations of the specifically disciplinary, since the 'discipline' derived from them can be fitted to 'the discipline', and since 'the discipline' is located in the disciplinary more generally, the ordering at any scale is a fractal of those at others. That effect allows a resolution of the puzzle of the recurrence of 'Mannheim's paradox' in sociological knowledge. Sociological knowledge is achieved through political and rhetorical appeals to the

common sense denied in its epistemologically foundational claims. Sociology as the distinctively disciplinary rests on failing attempts to sustain its distinctiveness. The discipline's with-world entails specific topics, a specialised vocabulary, and particular folk heroes, but, singly and as an ensemble, the essentially contestable character of the topics, the polysemic effectivity of the vocabulary, and the arationality of appeals to folk heroes ensure the continued failure of disciplinary closure through a continued reliance on popular common sense. On this epistemically flat account, sociological knowledge is possible because of, rather than despite, the unsustainability of foundational claims made for it.

Now since the derivation of 'fractal ordering' is itself a fractal ordering, this resolution of 'Mannheim's paradox' has the limits of its advantages. Through the use of antimetabole rather than antithesis, it does allow that self-exemplifying, non-foundational, and epistemically flat sociology required by the discipline's location in what is studied within it. It does allow a reading of the peculiar genre of presidential address. It does allow an alternative treatment of the standard sociological distinction between formal and informal. But all this is so only to a 'fairly coherent' extent. Since the thesis is itself an effect of ineluctably trans-scalar effectivities, it can suggest far more than it can resolve. That follows from the initial three postulates, and, indeed from the adoption of trinary ordering. While the paradigmatic standing of 'discipline' might be widely agreed, the uses of actor-network theory and of classical rhetoric entail related roads not taken.

The triadic structure of fractal ordering is a practical matter. Some principle of classification, some architectonic, is needed, and once dualism is problematised, an odd number of categories is preferable to an even. Ordering by threes is simply the most economical practice. It was not imposed on the readings of Scheler and Mannheim - both of them stressed their use of it - but for the rest of the thesis it was a practical convenience. So while it may have the metaphysical connotations given in 'trinitarianism', or while it may be linkable to the more pervasive cultural structures noted in Chapter 2, examination of those possibilities is well beyond this study.

The rhetorical and actor-network theoretical use of Bacon as a focal figure is just as pragmatically determined. He affords, as Comte (1830-42) said, a convenient rallying-point. He is genealogically located in the sociologies of knowledge; the

reversal of 'Mannheim's paradox' in his rhetorical denial of rhetoric is paradigmatic. His work is used in one conventional dating of the modern. His Salomon's House epitomises the imaginary of collectivity, science, and usefulness. The presidents did invoke him, both explicitly and tacitly, more than they did most other non-sociological writers, and more than any other philosopher or sociologist of science. But while that gave the thesis a thematic continuity, 'Bacon' is no more than a rhetorical topic, or a macroterm, in it. To open that black box would be another project.

The focus on the Baconian distinction between political and epistemological senses of representation was taken from Latour's definition of the modern as an effect of the purification which follows from denial of hybrids of nature-culture; the finding that purification could be sustained in neither the formal nor the folk sociologies of knowledge followed from his claim of inevitable failure. Since that was part of his argument that 'we have never been modern', the couching of the thesis in terms of classical rhetoric might imply a pathos of there being nothing new under the sun. That would be misleading. As Scheler (1925: 105; cf. Merton 1968a) said of the use of classical precedents: *Why now?* Although particular ancestors are retrogenerated at particular times under particular conditions, the trans-historical generality of 'fractal ordering' has been achieved in the thesis at the cost of that historical specificity on which sociologists of knowledge have always insisted. While Latour diagnosed 'purification' in relation to debates over the modern and the postmodern, and while 'fractal ordering' is relevant to those debates, that is another connection left undeveloped here.

The more general use in the thesis of actor-network theory is just as limited. The assumptions that knowledge is political and is ethnographically accessible, the opening of epistemological foreclosures, the relative coherence of trinary ordering, or the insistence on the material effectivity of textuality, are all either consistent with or taken from it. But the focus on textuality in the thesis means that the translation is limited. While the actor-network theorists have treated the co-emergence of knowledge and the social as effects of networks, hybrids of nature-culture, comprising documents, devices and drilled bodies and being simultaneously real like nature, narrated like discourse, and collective like society, and while the thesis was couched in those terms, 'documents' and 'narrated like discourse' have been privileged over the other two moments in each triad. So the thesis is then a selective enrolment of actor-network theory rather than a project within it. A more complete study of the networks

generating sociological knowledge would require a symmetrical degree of attention to practices beyond the textual.

The roads not taken in 'fractal ordering' are epitomised in the requirement to reconcile self exemplification with a modelling of 'discipline' as 'congenitally failing'. Clearly the thesis was not written to fail. Success is claimed for it as a description of previously unnoticed regularities in sociological practice, as a setting of continuities in the sociologies of knowledge, and as an account of the little-studied genre of presidential address. But since work within 'Mannheim's paradox' allows no more than a pragmatic resolution of its circularities, such success is a 'modest' demonstration of possibilities. The thesis follows from Mannheim's insistence that the difficulties in the sociology of knowledge be left open. It is an endorsement of the actor-network theorists' insistence that sociology is a teratology. It cannot be claimed as definitive. To 'fail' is to accept that knowledge is always already political.

In that sense, the principle of fractal ordering could be extended to link sociology to practices of governance under neo-liberalism, along the lines of the governmentality from which sociology was in part derived. Potential links with other sociological problematics entailing a contrast between formal and informal are also evident in the disciplinary rhetorics represented in the homology between Mannheim's, Kuhn's and the actor-network theorists' accounts of knowledge. Disciplined knowledge is a continually receding synthesis of liberal, conservative and socialist moments, an essential tension between symbolic generalisations, analogical models and lived exemplars, or an effect of networks which are real like nature, narrated like discourse, and collective like society. Fractal ordering, shown in the emergent, trans-scalar and jaggedly regular achievement of sociology from the elements of what is studied within it in those terms, is a template for further study.

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**APPENDIX A : PRESIDENTIAL ADDRESSES TO THE
ASS/A, 1935-1994, AS PUBLISHED IN THE *AMERICAN
SOCIOLOGICAL REVIEW***

Year	President	ASR	Title
1935	F. Stuart Chapin	1(1) 1936 1-11	Social theory and social action.
1936	Henry Pratt Fairchild	2(1) 1937 1-8	Business as an institution.
1937	Ellsworth Faris	3(1) 1938 1-12	The promise of sociology.
1938	Frank H. Hankins	4(1) 1939 1-15	Social science and social action.
1939	Edwin H. Sutherland	5(1) 1940 1-12	White-collar criminality.
1940	Robert M. MacIver	6(1) 1941 1-8	Some reflections on sociology during a crisis.
1941	Stuart A. Queen	7(1) 1942 1-12	Can sociologists face reality ?
1942	Dwight Sanderson	8(1) 1943 1-9	Sociology a means to democracy.
1943	George A. Lundberg	9(1) 1944 1-13	Sociologists and the peace.
1944	Rupert B. Vance	10(2) 1945 123-31	Toward social dynamics.
1945	Kimball Young	11(2) 1946 137-46	Society and the state : Some neglected areas of research and theory.
1946	Carl C. Taylor	12(1) 1947 1-9	Sociology and common sense.

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|------|--------------------------|----------------------|------------------------------------------------------------------------------------|
| 1947 | Louis Wirth | 13(1) 1948
1-14 | Consensus and mass communication. |
| 1948 | E. Franklin Frazier | 14(1) 1949
1-11 | Race contacts and the social structure. |
| 1949 | Talcott Parsons | 15(1) 1950
1-16 | The prospects of sociological theory. |
| 1950 | Leonard S. Cottrell, Jr. | 15(6) 1950
705-12 | Some neglected problems in social psychology. |
| 1951 | Robert C. Angell | 16(6) 1951
749-57 | Sociology and the world crisis. |
| 1952 | Dorothy S. Thomas | 17(6) 1952
663-9 | Experiences in interdisciplinary research. |
| 1953 | Samuel A. Stouffer | 18(6) 1953
591-7 | Measurement in sociology. |
| 1954 | Florian Znaniecki | 19(5) 1954
518-24 | Basic problems of contemporary sociology. |
| 1955 | Donald Young | 20(6) 1955
641-8 | Sociology and the practicing professions. |
| 1956 | Herbert Blumer | 21(6) 1956
683-90 | Sociological analysis and the 'variable'. |
| 1957 | Robert K. Merton | 22(6) 1957
635-59 | Priorities in scientific discovery: A chapter in the sociology of science. |
| 1958 | Robin M. Williams, Jr. | 23(6) 1958
619-33 | Continuity and change in sociological study. |
| 1959 | Kingsley Davis | 24(6) 1959
757-72 | The myth of functional analysis as a special method in sociology and anthropology. |
| 1960 | Howard Becker | 25(6) 1960
803-10 | Normative reactions to normlessness. |

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|------|--------------------|-----------------------|--------------------------------------------------------------------------|
| 1961 | Robert E. L. Faris | 26(6) 1961
835-43 | Reflections on the ability dimension
in human society. |
| 1962 | Paul F. Lazarsfeld | 27(6) 1962
757-67 | The sociology of empirical social
research. |
| 1963 | Everett C. Hughes | 28(6) 1963
879-90 | Race relations and the sociological
imagination. |
| 1964 | George C. Homans | 29(5) 1964
809-18 | Bringing men back in. |
| 1965 | Pitirim A. Sorokin | 30(6) 1965
833-43 | Sociology of yesterday, today and
tomorrow. |
| 1966 | Wilbert E. Moore | 31(6) 1966
765-72 | The utility of utopias. |
| 1967 | Charles P. Loomis | 32(6) 1967
875-90 | In praise of conflict and its resolution. |
| 1968 | Philip M. Hauser | 34(1) 1969
1-19 | The chaotic society : Product of the
social morphological revolution. |
| 1969 | Ralph H. Turner | 34(6) 1969
815-31 | The public perception of protest. |
| 1970 | Reinhard Bendix | 35(5) 1970
831-43 | Sociology and the distrust of reason. |
| 1971 | William H. Sewell | 36(5) 1971
793-809 | Inequality of opportunity for higher
education. |
| 1972 | William J. Goode | 37(5) 1972
507-19 | The place of force in human society. |
| 1973 | Mirra Komarovsky | 38(6) 1973
649-62 | Some problems in role analysis. |
| 1974 | Peter M. Blau | 39(5) 1974
615-35 | Parameters of social structure. |

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|------|---------------------|-----------------------|----------------------------------------------------------------------------------------------------|
| 1975 | Lewis A. Coser | 40(6) 1975
691-700 | Two methods in search of a substance. |
| 1976 | Alfred McClung Lee | 41(6) 1976
925-36 | Sociology for whom ? |
| 1977 | J. Milton Yinger | 42(6) 1977
833-53 | Countercultures and social change. |
| 1978 | Amos H. Hawley | 43(6) 1978
787-96 | Cumulative change in theory and in history. |
| 1979 | H. M. Blalock, Jr. | 44(6) 1979
881-94 | Measurement and conceptualization problems: The major obstacle to integrating theory and research. |
| 1980 | Peter H. Rossi | 45(6) 1980
889-904 | The challenge and opportunities of applied social research. |
| 1981 | William Foote Whyte | 47(1) 1982
1-13 | Social inventions for solving human problems. |
| 1982 | Erving Goffman | 48(1) 1983
1-17 | The interaction order. |
| 1983 | Alice S. Rossi | 49(1) 1984
1-19 | Gender and parenthood. |
| 1984 | James F. Short, Jr. | 49(6) 1984
711-25 | The social fabric at risk : Toward the social transformation of risk analysis. |
| 1985 | Kai Erikson | 51(1) 1986
1-8 | On work and alienation. |
| 1986 | Matilda White Riley | 52(1) 1987
1-14 | On the significance of age in sociology. |
| 1987 | Melvin L. Kohn | 52(6) 1987
713-31 | Cross-national research as an analytic strategy. |
| 1988 | Herbert J. Gans | 54(1) 1989
1-16 | Sociology in America : The discipline and the public. |

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|------|-----------------------|--------------------|-----------------------------------------------------------------------------------|
| 1989 | Joan Huber | 55(1) 1990
1-10 | Macro-micro links in gender stratification. |
| 1990 | William Julius Wilson | 56(1) 1991
1-14 | Studying inner-city social dislocations: The challenge of public agenda research. |
| 1991 | Stanley Lieberman | 57(1) 1992
1-15 | Einstein, Renoir, and Greeley: Some thoughts about evidence in sociology. |
| 1992 | James S. Coleman | 58(1) 1993
1-15 | The rational reconstruction of society. |
| 1993 | Seymour Martin Lipset | 59(1) 1994
1-22 | The social requisites of democracy revisited. |
| 1994 | William A. Gamson | 60(1) 1995
1-20 | Hiroshima, the Holocaust, and the politics of exclusion. |

APPENDIX B : REAL AND IDEAL FACTORS

Table B.1: Two ages of the presidents

(A, year of presidency; B, year of birth; C, year of doctorate; D, disciplinary age; E, chronological age at presidency)

	A	B	C	D	E
Chapin	1935	1888	1911	24	47
Fairchild	1936	1880	1909	27	56
E. Faris	1937	1874	1914	23	63
Hankins	1938	1877	1908	30	61
Sutherland	1939	1883	1913	26	56
MacIver	1940	1882	1905	35	58
Queen	1941	1890	1919	22	51
Sanderson	1942	1878	1921	21	64
Lundberg	1943	1895	1925	18	48
Vance	1944	1899	1928	16	45
K. Young	1945	1893	1920	25	52
Taylor	1946	1884	1918	28	62
Wirth	1947	1897	1926	21	50
Frazier	1948	1894	1931	17	54
Parsons	1949	1902	1929	20	47
Cottrell	1950	1899	1933	17	51
Angell	1951	1899	1924	27	52
D. Thomas	1952	1899	1924	28	53
Stouffer	1953	1900	1930	23	53
Znaniecki	1954	1882	1909	45	72
D. Young	1955	1896	1922	33	59
Blumer	1956	1900	1928	28	56
Merton	1957	1910	1936	21	47
Williams	1958	1914	1943	15	44
Davis	1959	1908	1936	23	51
Becker	1960	1899	1930	30	61
R. Faris	1961	1907	1931	30	54
Lazarsfeld	1962	1901	1927	35	61
Hughes	1963	1897	1928	35	66
Homans	1964	1910	[1939]	25	54

Table B.1: Continued (A, year of presidency; B, year of birth; C, year of doctorate; D, disciplinary age; E, chronological age at presidency)

	A	B	C	D	E
Sorokin	1965	1889	1922	43	76
Moore	1966	1914	1940	26	52
Loomis	1967	1905	1933	34	62
Hauser	1968	1909	1938	30	59
[Turner]	1969	1919	1948	21	50
Bendix	1970	1916	1947	23	54
Sewell	1971	1909	1939	32	62
Goode	1972	1917	1946	26	55
Komarovsky	1973	1906	1940	33	67
Blau	1974	1918	1952	22	56
Coser	1975	1913	1954	21	62
Lee	1976	1906	1933	43	70
Yinger	1977	1916	1942	35	61
Hawley	1978	1910	1941	37	68
Blalock	1979	1926	1954	25	53
P. Rossi	1980	1921	1951	29	59
Whyte	1981	1914	1943	38	67
Goffman	1982	1922	1953	29	60
A. Rossi	1983	1922	1957	26	61
Short	1984	1924	1951	33	60
Erikson	1985	1931	1962	23	54
Riley	1986	1911	[1940]	46	75
Kohn	1987	1928	1952	35	59
Gans	1988	1927	1957	31	61
Huber	1989	1925	1967	22	64
Wilson	1990	1935	1965	25	55
Liebertson	1991	1933	1960	31	58
Coleman	1992	1926	1955	37	66
Lipset	1993	1922	1949	44	71
Gamson	1994	1934	1959	35	60

Table B.2: Schools at which presidents earned their doctorates.

Chapin/1935	Columbia	Sorokin/1965	Leningrad
Fairchild/1936	Yale	Moore/1966	Harvard
E. Faris/1937	Chicago	Loomis/1967	Harvard
Hankins/1938	Columbia	Hauser/1968	Chicago
Sutherland/1939	Chicago	Turner/1969	Chicago
Madver/1940	Edinburgh	Bendix/1970	Chicago
Queen/1941	Chicago	Sewell/1971	U. Minnesota
Sanderson/1942	Chicago	Goode/1972	Penn. State
Lundberg/1943	Minnesota	Komarovsky/1973	Columbia
Vance/1944	U. N'th C'lina	Blau/1974	Columbia
K. Young/1945	Stanford	Coser/1975	Columbia
Taylor/1946	U. Missouri	Lee/1976	Yale
Wirth/1947	Chicago	Yinger/1977	U. Wisconsin
Frazier/1948	Chicago	Hawley/1978	U. Michigan
Parsons/1949	Heidelberg	Blalock/1979	U. N'th C'lina
Cottrell/1950	Chicago	P. Rossi/1980	Columbia
Angell/1951	U. Michigan	Whyte/1981	Chicago
D. Thomas/1952	LSE	Goffman/1982	Chicago
Stouffer/1953	Chicago	A. Rossi/1983	Columbia
Znaniecki/1954	Cracow	Short/1984	Chicago
D. Young/1955	U. Pennsylvania	Erikson/1985	Chicago
Blumer /1956	Chicago	Riley/1986	[Harvard]
Merton/57	Harvard	Kohn/1987	Cornell
Williams/1958	Harvard	Gans/1988	U. Penn.
Davis/1959	Harvard	Huber/1989	Michigan State
Becker/1960	Chicago	Wilson/1990	Wash'ton State
R. Faris/1961	Chicago	Liebersson/1991	Chicago
Lazarsfeld/1962	Vienna	Coleman/1992	Columbia
Hughes/1963	Chicago	Lipset/1993	Columbia
Homans/1964	[Harvard]	Gamson/1994	U. Michigan

Table B.3: 'Embeddedness' of the presidents of the ASS/A, 1935-1994

(V, vice-presidency, scores 3 ; S, secretary, scores 2 for each year; C, council, scores 1 for each year; Em, 'embeddedness')

President	V	S	C	Em
Chapin/1935	-	-	6	6
Fairchild/1936	3	-	-	3
E. Faris/1937	3	-	3	6
Hankins/1938	3	-	-	3
Sutherland/1939	3	-	4	7
MacIver/1940	-	-	3	3
Queen/1941	3	-	4	7
Sanderson/1942	3	-	1	4
Lundberg/1943	-	-	3	3
Vance/1944	-	-	2	2
K. Young/1945	3	-	7	10
Taylor/1946	3	-	2	5
Wirth/1947	-	*	2	2
Frazier/1948	3	-	4	7
Parsons/1949	-	10	-	10
Cottrell/1950	3	-	2	5
Angell/1951	3	-	1	4
Thomas/1952	6	-	6	12
Stouffer/1953	-	-	4	4
Znaniecki/1954	-	-	3	3
D. Young/1955	-	6	6	12
Blumer /1956	3	*	4	7
Merton /1957	3	-	3	6
Williams/1958	3	6	4	13
Davis/1959	3	-	5	8
Becker/1960	-	-	4	4
R. Faris/1961	3	-	5	8
Lazarsfeld/1962	-	-	-	-
Hughes/1963	-	-	3	3
Homans/1964	3	-	2	5

* Wirth and Blumer both served as secretary of the ASS before the break with Chicago.

Table B.3: Continued (V, vice-presidency, scores 3 ; S, secretary, scores 2 for each year; C, council, scores 1 for each year; Em, 'embeddedness')

President	V	S	C	Em
Sorokin/1965	-	-	-	-
Moore/1966	3	-	-	3
Loomis/1967	-	-	3	3
Hauser/1968	3	-	6	9
Turner/1969	[3]	-	3	3
Bendix/1970	3	-	6	9
Sewell/1971	3	-	5	8
Goode/1972	3	-	3	6
Komarovsky/1973	3	-	3	6
Blau/1974	-	-	3	3
Coser/1975	-	-	3	3
Lee/1976	-	-	-	-
Yinger/1977	-	6	3	9
Hawley/1978	-	-	3	3
Blalock/1979	-	-	3	3
P. Rossi/1980	-	6	3	9
Whyte/1981	-	-	3	3
Goffman/1982	-	-	-	-
A. Rossi/1983	3	-	-	3
Short/1984	-	6	3	9
Erikson/1985	-	-	3	3
Riley/1986	3	-	3	6
Kohn/1987	-	-	3	3
Gans/1988	-	-	3	3
Huber/1989	3	-	3	6
Wilson/1990	-	-	-	-
Lieberson/1991	-	-	3	3
Coleman/1992	-	-	6	6
Lipset/1993	-	-	3	3
Gamson/1994	-	-	6	6

Table B.4: Vice-Presidents of the ASS/A, 1935-98

(The asterisk marks those subsequently elected to the presidency)

1935	Arthur J. Todd	1965	Robert Bierstedt
1936	Dwight Sanderson *	1966	[Arnold M. Rose *]
1937	C.S. Johnston	1967	Rudolf Heberle
1938	Warren Thompson	1968	William J. Goode *
1939	Dorothy S. Thomas *	1969	[Ralph H. Turner *]
1940	Stuart A. Queen *	1970	Gerhard E. Lenski
1941	J.H.S. Bossard	1971	Morris Janowitz
1942	Harold A. Phelps	1972	Mirra Komarovsky *
1943	Kimball Young *	1973	Raymond W. Mack
1944	Read Bain	1974	Matilda White Riley *
1945	Carl C. Taylor *	1975	Neil Smelser *
1946	Leonard S. Cottrell *	1976	Alex Inkeles
1947	E. Franklin Frazier *	1977	Suzanne Keller
1948	Robert C. Angell *	1978	Alice S. Rossi *
1949	Dorothy S. Thomas *	1979	Charles Y. Glock
1950	Robert K. Merton *	1980	Helen MacGill Hughes
1951	Margaret Jarman Hagood	1981	Renee C. Fox
1952	Clifford Kirkpatrick	1982	Joan Huber *
1953	Herbert Blumer *	1983	Everett K. Wilson
1954	Jessie Bernard	1984	Edgar F. Borgatta
1955	Philip M. Hauser *	1985	Morris Rosenberg
1956	Robin M. Williams *	1986	Rose Laub Coser
1957	Kingsley Davis *	1987	Mayer N. Zald
1958	Robert E.L. Faris *	1988	Richard J. Hill
1959	Harry Alpert	1989	Glen H. Elder
1960	Wilbert E. Moore *	1990	Edna Bonacich
1961	George C. Homans *	1991	Barbara Reskin
1962	William H. Sewell *	1992	Doris K. Wilkinson
1963	Leonard Broom	1993	Jill S. Quadagno *
1964	Reinhard Bendix *	1994	Barrie Thorne

		1995	Karen S. Cook
		1996	Myra Marx Ferree
		1997	Charles Willie
		1998	Cora Bagley Marrett

Table B.6: Presidents of the ASS/A 1935-98
 (The asterisk marks those previously vice-president)

1935	Chapin	1955	D. Young	1975	Coser
1936	Fairchild *	1956	Blumer *	1976	Lee
1937	E. Faris *	1957	Merton *	1977	Yinger
1938	Hankins *	1958	Williams *	1978	Hawley
1939	Sutherland *	1959	Davis *	1979	Blalock
1940	MacIver	1960	Becker	1980	P. Rossi
1941	Queen *	1961	R. Faris *	1981	Whyte
1942	Sanderson *	1962	Lazarsfeld	1982	Goffman
1943	Lundberg	1963	Hughes	1983	A. Rossi *
1944	Vance	1964	Homans *	1984	Short
1945	K. Young *	1965	Sorokin	1985	Erikson
1946	Taylor *	1966	Moore *	1986	Riley *
1947	Wirth	1967	Loomis	1987	Kohn
1948	Frazier *	1968	Hauser *	1988	Gans
1949	Parsons	1969	Turner *	1989	Huber *
1950	Cottrell *	1970	Bendix *	1990	Wilson
1951	Angell *	1971	Sewell *	1991	Lieberson
1952	D. Thomas *	1972	Goode *	1992	Coleman
1953	Stouffer	1973	Komarovsky *	1993	Lipset
1954	Znaniecki	1974	Blau	1994	Gamson

				1995	Etzioni
				1996	Hallinan
				1997	Smelser *
				1998	Quadagno *

Table B.7: Highest (descending order) and lowest (ascending order) five users of terms and repertoires in the content-analysis. (Low users are omitted when five or more presidents scored zero).

<u>First Person Singular</u>		<u>First Person Plural</u>	
High	Low	High	Low
Thomas 1952	R. Faris 1961	MacIver 1940	Sutherland 1939
Loomis 1967	Turner 1969	Gans 1988	Huber 1989
Lazarsfeld 1962	Williams 1958	Queen 1941	Wilson 1990
Parsons 1949	Sutherland 1939	E. Faris 1937	Lipset 1993
Cottrell 1950	D. Young 1955	Cottrell 1950	Taylor 1946

<u>Vocabulary</u>		<u>Sociology</u>	
High	Low	High	Low
Znaniecki 1954	E. Faris 1937	Gans 1988	Hawley 1978
Blau 1974	Erikson 1985	D. Young 1955	Wilson 1990
Davis 1959	Sutherland 1939	Lee 1976	Sutherland 1939
Sorokin 1965	Bendix 1970	Sorokin 1965	Lipset 1993
Vance 1944	Lieberson 1991	Fairchild 1936	Gamson 1994

<u>Science</u>		<u>Empiricist</u>	
High	Low	High	Low
Merton 1957	Sutherland 1939	Blumer 1956	Hankins 1938
Lundberg 1943	Turner 1969	P. Rossi 1980	Gamson 1994
Bendix 1970	Blau 1974	Kohn 1987	Hughes 1963
Taylor 1946	Gamson 1994	Parsons 1949	Wirth 1947
D. Young 1955	Sewell 1971	Sorokin 1965	Hauser 1968

Interpretivist

High

Low

Kohn 1987

Hauser 1968

Blumer 1956

Lipset 1993

Turner 1969

Hughes 1963

Frazier 1948

Sutherland 1939

Sewell 1971

Hankins 1938

Usefulness

High

Low

D. Young 1955

Becker 1960

P. Rossi 1980

Loomis 1967

Cottrell 1950

Blau 1974

Whyte 1981

Erikson 1985

Taylor 1946

Lipset 1993

Normativity

High

Low

Gans 1988

Riley 1986

Sewell 1971

Short 1984

Cottrell 1950

Sorokin 1965

MacIver 1940

Wilson 1990

Chapin 1935

A. Rossi 1983

Location

High

Low

Hauser 1968

Taylor 1946

Queen 1941

Goffman 1982

Gans 1988

Hawley 1978

R. Faris 1961

Davis 1959

Hughes 1963

Vance 1944

KnowledgeDisciplineRepresentModern

High

High

High

High

Taylor 1946

Thomas 1952

K. Young 1945

Loomis 1967

Sorokin 1965

Gans 1988

Blumer 1956

Erikson 1985

D. Young 1955

P. Rossi 1980

MacIver 1940

Wirth 1947

Bendix 1970

Lee 1976

Blalock 1979

Znaniecki 1954

MacIver 1940

Bendix 1970

Sorokin 1965

Bendix 1970